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EDINBURGH NEW
PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

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THE

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PHILOSOPHICAL JOURNAL.

Notes on the Total Eclipse of the Sun of July 18, 1860, *as observed in Spain.* By Captain W. S. JACOB, late Director of the Madras Observatory. (Plate I.)

THE writer of these notes was one of the party who accompanied the astronomer royal to the north of Spain in the Himalaya, the steamer liberally provided by government for the conveyance of such astronomers as desired to observe the eclipse, which would be total there, and in no other part of Europe. The greater portion of the expedition landed at Bilbao, and then dispersed themselves in small parties along the line which the moon's shadow would traverse, selecting the most favourable spots, according to the best information procurable, in which they met with invaluable assistance from C. Vignoles, Esq., the chief engineer of the Bilbao and Tudela Railway, whose operations had made him well acquainted with the country, and who was at the pains of printing a pamphlet containing all the information likely to be useful, accompanied by a map, on a large scale, of the country embracing the track of the shadow.

Our party consisted of Professor Grant and Dr M'Taggart, both of Glasgow, and myself, accompanied by Mr Martin, C.E., one of the engineers of the railway, who accompanied us as interpreter and guide, and materially assisted all our arrangements; for it should be stated, that through the liberality of the directors, each party was accompanied by an engineer, or other functionary of the railway, used to the ways of the country and speaking its language. The point which we had selected was the pass of Peñacerráda, where the road from Vitoria to Logroño crosses the Sierra de Toloño, a mountain ridge of some length, the summits of which rise to between 4000 and 5000 feet in height. Here we had been led to expect, almost certainly, an unclouded sky. But the derangement of climate, which we have experienced in the north, has partly extended also to Spain; and on our arrival, on the morning of 17th July, at the village of Peñacerráda, where the ascent of the mountain commences, we were dismayed at finding no improvement in the weather; on the contrary, the clouds reached almost to the foot of the hills, so that our purpose was quite frustrated of selecting a suitable spot for our instruments, getting them into adjustment, fixing latitude and time, &c., so that there might be nothing to distract our attention on the day of the eclipse; for on the hill we could not see five yards before us. There being no remedy, we returned to the village inn, to wait with patience till the next morning. Our advent caused some stir in the place, and we had a call in the afternoon from the Cura (or parson), a mild, respectable looking old gentleman of near seventy, who, in the absence of our interpreter, conversed as well as he could in Latin with Dr M'Taggart, our stock of Spanish being very limited; but on Mr Martin coming in, we drew from him some reminiscences of the battle of Vitoria, a portion of the English forces having encamped near the village on the day preceding the battle; and he spoke with affectionate respect of the Duke of Wellington, calling him the father of Spain. At Vitoria, on the contrary, we found the inhabitants rather French in their sympathies, and for the most part ignorant of the fact of a decisive battle having been fought in their neighbourhood.

The morning of the 18th dawned with prospects but slightly improved; the sky still quite overcast, though the clouds were higher on the hills than yesterday; and as we began to ascend about 6 A.M., they gradually rose above us; and when we had reached the summit of the pass, about 3500 feet in height, they were still above our heads, but perched on the mountain peaks. Just beyond the top of the pass a magnificent view awaited us; we looked down on the valley of the Ebro, winding its way among vineyards and corn fields, about 2000 feet below us; southward there was a clear view to the mountains of Burgos, some thirty miles distant, with their heads still in the clouds, while along the line of the river the eye could sweep from the hills of Pancorbo in the north-west to near Tudela in the south-east, a range of more than eighty miles. Many towns and villages were discerned, among which were Briones—which though distant eight miles, seemed almost at our feet—Haro, Logroño, and La Guardia. Patches of sunlight could be seen in the distance, on beholding which, some of us were for pressing on immediately and descending to the plains, under the impression that the clouds were clinging to the mountains; but were induced to wait by the suggestion that our mules needed rest, and that there would be abundant time to move on some hours later should the weather not improve, while by leaving our vantage-ground we should lose some of the most interesting phenomena, viz., the effects on the distant landscape. We therefore took up a position, on a rocky ledge jutting out over the valley about 500 feet below the top of the pass, directly above the village of Leza, and were gratified by seeing the sun break out a little before ten. We were thus enabled roughly to fix our time and latitude, which latter was found to be 42° 34′. Towards the commencement of the eclipse the sky had quite cleared; the first contact was noted at $1^h\ 48^m\ 32^s$ (approximate Greenwich time); at $2^h\ 30^m$, the light and heat had sensibly decreased, and the former had taken a decidedly yellow tinge; at $2^h\ 44^m$, Venus was caught in the telescope as the finest possible crescent, being very near conjunction; at $2^h\ 55^m$, she was seen with the naked eye, and Jupiter about 2 minutes later. At $2^h\ 52^m$, several tabular mountains on the moon's limb were noticed near the south cusp, giving it a jagged appearance. At $2^h\ 57^m$, long coloured rays, like those of inflected light, were seen proceeding *outwards*, or to the left from the sun, now become quite a thin crescent. At $2^h\ 58^m$, fine parallel lines of shadowy waves running nearly north and south, were seen moving rapidly eastward over the rocks, accompanied by a flickering in the air. The light had now become very dim; an inky gloom prevailed up the valley westward, and a sickly yellowish hue eastward, while in the plain below, the shadows of small passing clouds seemed unusually black and sharply defined. At $3^h\ 1^m\ 0^s$, the sun disappeared, the corona having suddenly flashed out ten seconds previously on the opposite side of the moon, and spread almost instantly round the circumference like the opening of a fan. In the telescope four prominences were seen, not at all resembling either flames or clouds, but nearly colourless, barely tipped with rosy points, and having clear definite outlines; one of them near the vertex looked something like a tree, or rather a cabbage; another a little to the left was quite detached; those on the left side gradually disappeared by the advance of the moon; one of these was shaped like a mitre, and as it sank, another, almost precisely similar, emerged at the opposite point; this last continued visible a short time after the sun's reappearance, which took place at $3^h\ 4^m\ 23^s$, without any trace of Baily's beads; the corona was visible about 12 seconds longer. During the obscuration, Jupiter, Venus, Mercury, and Saturn, were well seen, as also Regulus and Castor. Round the horizon a deep orange tint prevailed to the height of some 10°, against which the distant hills showed very black, while the low clouds looked rather of a dirty green, somewhat like tarnished brass. The shadow passed away down the valley rather like a faint shower, but paler and more uniform. The light during the total obscuration was certainly much greater than that of the full moon, for the watch could be read with ease, but the light was of quite a different character from moonlight, resembling more the gloom of a heavy thunderstorm after sunset. About $3^h\ 7^m$, there was a repetition of the shadowy lines and flickering light, the cause of which was not apparent. The hue of the corona rather resembled dead silver, and there were rays proceeding from it about twenty in number, but of irregular lengths and intervals, the longest extending near 30′. A number of persons from the village had collected about us soon after the commencement of the eclipse, and neither they nor the animals which accompanied them (mules and dogs) showed any signs of emotion or alarm at the coming on of the darkness; yet I must confess to have been conscious myself of a certain undefinable feeling of awe, arising no doubt from the strange aspect of things, and the unusual garb which all nature seemed to have put on; the livid gloom that seemed pouring down from the sky; the purplish-black of the mountains, which seemed to draw them closer to us; and above all, that strange black spot in the heavens with the mild glory round it; all conspired to excite a sense of wonder, even though the mind was prepared for some such phenomena. The prominences were not seen by any of our party with the naked eye, nor during the time they were watched was any change discernible in their forms, beyond what was caused by their being covered or uncovered by the moon.

The country through which we passed impressed us favourably. The productions, unlike those of southern Spain, much resembling our own, the greater warmth of climate being indicated by the luxuriance of the wild plants and flowers, the forwardness of the harvest, which had already commenced, and especially by the way in which the vines flourished in the open air, though no *large* vineyards were seen excepting near the Ebro. The Rioja wine, made in the upper part of the Ebro valley, is of good quality, somewhat resembling Burgundy, and would probably find favour in England if better known; it can be purchased at about 1s. per gallon. The people seemed physically a fine race, the women and children being remarkably handsome; but though somewhat less under priestly domination than formerly, they are still feeling its effects, and with some considerable degree of *civil*, there seems to be very little religious freedom as yet, no worship but the Roman Catholic being tolerated. We were not troubled by any custom-house inspection either on arriving or leaving, nor were our passports once asked for. The accompanying diagrams (Plate I.) show—No. 1, the appearance of the corona to the naked eye, as drawn from memory the day after the eclipse.* The places of the prominences are just indicated, though they were not seen with the naked eye. No. 2 shows the forms and positions of the prominences, as referred to the northern point of the limb, which is the lowest in the diagram, and as they appeared about 30 seconds after commencement of totality, excepting *d*, which is shown as it appeared just before the sun's reappearance.

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* The light was rather more diffuse, and the rays less hard and distinct, than the print represents them.

On the Eruption, in May 1860, *of the Kötlugjá Volcano, Iceland.* By W. LAUDER LINDSAY, M.D., F.L.S. (Plate II.)

On the 6th June last I went on board the "Arcturus," Danish mail screw steamer, which was then at Grangemouth *en route* from Copenhagen to the Faröe Islands and Iceland. Captain Andresen, its commander, informed me as the latest news from Iceland, that the Kötlugjá volcano, after a quiescence of thirty-seven years, had just burst out afresh. He expressed a hope that his passengers might have an opportunity of seeing it in eruption; and promised, with a view to their doing so, to run as close as he safely could along the south coast of Iceland, about 20 miles inland, and to the north of which the said volcano is situated. On the 8th we sailed from Grangemouth; on the 11th we touched at Thorshavn, the capital of Faröe. Early in the morning of the 13th we sighted the south coast of Iceland, in the direction of the Oræfa-jökul; and on the afternoon of the same day we ran close along the coast between Portland Huk and the Westmanna Islands. The weather was magnificent, the sky comparatively serene; and we had therefore the best, and—perhaps for Iceland,—unusual, opportunities of scanning the whole range of the southern Jökuls,* from the celebrated Hekla in the west, to the more imposing Oræfa in the east, and particularly the range of the Eyafjalla and Myrdals jökuls, of which latter Kötlugjá is a part. At one time some of the more sanguine passengers fancied they saw a column of smoke arising in the direction of Kötlugjá; but this soon proved to be merely a passing cloud of pseudo-columnar form, which happened for a few moments to rest on that part of the shoulder of Myrdals-jökul, behind which we knew Kötlugjá to be situate. The cloud in due time floated past, and all was again serene; not

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* The word Jökul is generally translated *Glacier*, or glacier-covered mountain. Henderson† (in a foot-note, vol. i. p. 39) says, it "signifies an *ice-mountain*, and is derived from Jaki, a lump or fragment of ice." The Icelandic Jökul, however, so far as I could ascertain, is simply a mountain generally covered more or less throughout the year with snow. I say deliberately *generally*, and *more or less*; for under certain circumstances,—*e.g.*, during very warm summers, or in their southern exposures,—some of those mountains may be sometimes stripped partially, or perhaps even wholly, of their snows. On the 13th June, when I passed Eyafjalla and Myrdals jökuls, there was almost no snow on the tops of the latter, and not a great deal on the shoulder of the former. Only here and there on Eyafjalla did the snow extend any distance down the mountain, while the whole of the lower parts of

† *Vide* foot-note page 9 of this article.

Myrdals-jökul were uncovered. This sparse covering was the more noteworthy, inasmuch as the southern outliers of the Perthshire Grampians,—hills of only about 1000 to 1800 feet of elevation, had still a coating of snow on their summits when I left Scotland, which coating did not altogether disappear from some of them till the middle of July! There was also a little snow on the tops of the highest of the Faröese hills on 11th June. The side of Eyafjalla and Myrdals jökuls exposed to our view was however the southern or warmer; and it is quite probable that the northern side of both was more plentifully shrouded. In Iceland, as in other mountainous countries, it does not always happen that the highest mountains are most plentifully covered with snow. At the time of my visit to Iceland, during June, Snæfells-jökul, on the west coast, which is probably the most handsome and striking of all the Icelandic jökuls, and which is quite visible from Reykjavik, forming the western termination of the hilly coast of the Faxéfiord, appeared covered with snow to its base. Its height, however, is only 4577 Danish feet, and it is therefore inferior in point of elevation to Eyafjalla (5432 feet), Hekla (4961 feet), and Oræfa (6241), all of which had a smaller covering of snow. So far as I am aware, there are few *true glaciers* in Iceland, what Gunnlaugsson calls *Skridjöklar* ("*Glaciers mouvants*"). The most familiar are southern offshoots of the extensive Vatnajökul or Klofa-jökul, which is also situated in the south of Iceland, not very distant from Myrdals-jökul, more to the east, and intermediate between it and Oræfa-jökul. The best known and largest of these glaciers are two, viz., the Skeidarar-jökul, and the Breidamerkr-jökul. Excellent descriptions of these—the fullest and most reliable I know—will be found in Henderson, vol. 1, pp. 237 and 265.

I believe the glaciers in question, and the vast snow-fields from which they spring, indeed the whole range of Jökuls from Hekla to Oræfa, with the lowland intervening between them and the sea, would richly repay investigation or exploration by any of our geologists. "The phenomena of these vacillating jökuls," says Henderson, who himself visited them (vol. i. p. 268), "would receive much elucidation from a survey of the situation and appearance of those parts of them, which lie towards the interior of the island; but the dangers connected with every attempt to explore them are more than sufficient to damp the zeal, and check the enterprise, of the most impassioned lovers of Natural History." (?) The main difficulty is how to get at them from Reykjavik, which latter is easily reached from Scotland (*vide* "Iceland a new field for Tourists," *Perthshire Advertiser*, August 2d and 9th 1860; "Recent Eruption of the Kötlugjá Volcano, Iceland," *Daily Scotsman*, Aug. 16, or, *Glasgow Daily Herald*, a vestige of any eruption, present or past, was to be seen from the deck of our steamer. To the tourists on board, who were mostly naturalists,* this was a source of disappointment; to the Icelanders and Danes,† it was, on the contrary, a cause of heartfelt gratification. The latter gentlemen naturally evinced great anxiety on the subject of the reported eruption; they anticipated with alarm the narrative of its devastations. There had been no volcanic eruption in Iceland since that of Hekla in 1845–6. It was alleged that as yet Kötlugjá had emitted only water; but this, it was feared, was only a prelude to the more destructive ejections of lava. The season of the year was most unfortunate, too,—the winter had been long and severe, and the grass was only now springing up; if the pasturages failed over any extent of country, the destruction of horses, sheep, and cattle, would be enormous, these animals being the mainstays of the poor inhabitants. Such anxieties and forebodings were however destined to agreeable disappointment.

Aug. 17, 1860.) But a yacht expedition would render such investigations comparatively easy and cheap. A party of naturalists, for instance, clubbing together, might hire a steam-yacht for two or three months during summer, say during June, July, and August. They would carry their own apparatus and provisions, tents, bedding, and horse-gear, with men to attend thereto. They should engage one or more Icelanders, experienced in Icelandic travelling, to act as interpreters, treasurers, and caterers for guides, quarters, ponies, and provisions; by this means only would innumerable difficulties be avoided. The yacht could land the party at various points on the coast, where ponies and guides could be got for excursions of 50 or 100 miles (*i.e.*, of two or three days' duration) into the interior. Those of the party, who contented themselves with daily excursions, might sleep most comfortably on board their yacht; while the others would require to be independent of all human habitations, and sleep in their tents amid scenes, than which it is impossible to conceive anything wilder or grander.

* Among them was a party of four, two young Germans, a Swiss, and an Orkneyman, who took with them tents, horse-gear, photographic apparatus, and appliances for collections, more especially in mineralogy, entomology, and ornithology. They subsequently spent two months in Iceland, visiting chiefly the most interesting district around the Myvatn in the north-east of the island. There was also a Welsh copper smelter and mine proprietor.

† Among the latter were the excellent Governor of Iceland, the Count von Trampe, whose *bonhommie* and intelligence rendered him a most agreeable and serviceable *compagnon de voyage*; one of the Sysselmen or sheriffs of districts (*vide* foot-note, p. 35), with his wife; one of the foreign consuls of Reykjavik; several of the principal merchants of Reykjavik and Havnafiord; and a lieutenant in the Danish army.

On the same evening, the 13th, we sailed through the group of the Westmanna Islands, where we landed a mail, and here we ascertained that the eruption of Kötlugjá had ceased about the end of May, having done comparatively little damage.

At Reykjavik, the capital of Iceland, which we reached on the 14th, and where I spent eight days, I made special inquiries as to the details of the eruption of Kötlugjá; but the amount of information I obtained fell very far short of what I had expected to collect. I found a narrative in one of the Reykjavik newspapers—the "Islendingur;"* and Dr Hjaltalin, the Physician-general of Iceland, drew up a similar narrative in English, which he requested me to get inserted in some of our Scotch newspapers, and which accordingly appeared at full length, and as it was put into my hands, in the "Perthshire Advertiser."† These narratives, so far as they relate to the eruption of May 1860, are exceedingly meagre, and are founded on evidence—of a most vague and unsatisfactory kind—of persons resident in the south of Iceland, who had seen the eruption only from a distance, and who happened subsequently to visit Reykjavik. These published accounts consist mainly, however, of a chronological history of *former* eruptions—this history being founded on, or derived from, old Sagas and other written or printed materials in the Public Library of Reykjavik, and being therefore likely to be accurate. But, in point of fulness and interest, where both accounts refer to the same eruptions, the narrative of our countryman Henderson‡ is infinitely superior. I was unsuccessful during my stay in Iceland in meeting with any one who had actually witnessed the Kötlugjá eruption for himself, and could give a coherent and reliable account thereof. There is, speaking generally, both as to the present and the past, a great lack of information useful to the man of science, as regards many of the most interesting local volcanic phenomena of Iceland. This depends on a variety of causes. While many Icelanders are devoted to the cultivation of history and poetry—in which departments of literature they undoubtedly excel—few or none seem to be equally attached to the natural sciences.[*] There is lacking, therefore, the enthusiasm and the knowledge necessary to the study of local geological phenomena. There may be said to be few, if any, naturalists [†] proper in Iceland. The best natural history surveys [‡] have been made by foreigners—Frenchmen, Germans, Danes, and Englishmen. Further, it must be stated in extenuation or excuse, that journeys in Iceland, especially during the winter, spring, and autumn seasons, are not only attended with difficulty, hardship, and danger, but with considerable expense. The latter barrier may be the more powerful, inasmuch as the cultivators of literature or science among the Icelandic people are not overburdened with money. Surveys of Iceland by Icelanders have generally been undertaken at the expense and instigation of the Danish Government, *e.g.*, that of twelve years' duration by Professor Björn Gunnlaugsson, teacher of Natural Philosophy and Mathematics in the Government College at Reykjavik (whose acquaintance I had the pleasure of making at the latter town), which resulted in the publication of the finest and most accurate map of Iceland extant.[§] It is evident, then, that a volcanic eruption at a distance of 80 or 100 miles from the capital is not a phenomenon likely to attract visitors from Reykjavik, or to induce them to undergo the tediousness, peril, and expense of Icelandic travelling—the object to be attained being apparently but a slight increment to geological knowlege. We are therefore generally dependent, for information as to local volcanic phenomena in Iceland, on the testimony of persons living near the localities concerned. Occasionally these persons are local clergymen or physicians, and it is fortunate for history and science when it is so; for these men being highly educated, and able to separate fact from fiction—the evidence of their senses from the *omne ignotum pro magnifico* of superstitious, ignorant, or terror-stricken peasants—are usually trustworthy witnesses. Since my return to Scotland, I have received from my Reykjavik friends files of newspapers published there subsequently to my departure; and in the "Islendingur" for 19th and 26th July (Nos. 8 and 9), I find a most interesting diary kept during the recent eruption, from 8th to 28th May 1860 by Sjera (Rev.) Magnús Hákonarson, clergyman at Reynir and Höfdabrékka in Myrdal. It does not always happen, however, that the historians of volcanic eruptions in Iceland are such trustworthy and competent witnesses. Too frequently the sole witnesses are peasants living at the base of the volcano, whose houses and herds are devastated perhaps by the first water-flood or shower of ashes, and who immediately flee in alarm and despair—panic-struck and poverty-struck—either exaggerating or misinterpreting actual phenomena, or, what is quite as likely, allowing their imagination to picture phenomena that never occurred. It is necessary to bear all this in mind in reading the history of volcanic

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* Islendingur of 5th and 16th June 1860, Nos. 5 and 6: "Eldgos úr Kötlugjá."

† Perthshire Advertiser, or Strathmore Journal, for 16th August 1860: "Recent Eruption of the Kötlugjá Volcano, Iceland."

‡ Iceland; or, a Journal of a Residence in that Island during the Years 1814–15. By the Rev. Ebenezer Henderson, D.D., Ph.D., Missionary of the British and Foreign Bible Society: 8vo. Edin., 1818; illustrated with a Map and Engravings. This work I can confidently commend as the best *British* work on Iceland hitherto published; specially valuable from its author having himself visited the greater part of the island, and come personally in contact with the most celebrated men therein.

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[*] It is but fair to state that an opposite opinion will be found recorded in the Edinburgh Cabinet Library volume on Iceland, p. 217.

[†] *Vide* foot-note, p. 31.

[‡] Of these, none have been conducted on so magnificent a scale and so exhaustive a plan as the voyage of "La Récherche," under the auspices of the French Government: "Voyage en Islande et au Grœnlande exécuté pendant les années 1835 and 1836, sur la Corvette La Récherche;" 6 vols. Paris, 1840, folio, with a magnificent atlas; edited by Dr Paul Gaimard. It contains a chapter by M. Robert on Icelandic Volcanoes (and on Icelandic geology and mineralogy generally), and is altogether by far the finest work on Iceland that has hitherto appeared. Its expensiveness, however, places it quite out of the reach of ordinary readers.

[§] "Uppdráttr Islands (Carte d' Islande) á fjórum blődum, (en quatre feuilles) gjördr ad fyrirsögn Olafs Nikolas Olsens (exécutée sous la direction de M. O. N. Olsén) gefinn út af enu Islenzka Bókmentafelagi (publiée par la Société littéraire d' Islande:") Reykjavik and Copenhagen, 1844; scale of

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about 6-8 miles to the inch. Price about 16s. in Reyjavik, 30s. in Britain. At the time when this map was published, I know of no country in the world that had been so minutely surveyed, and of no map so beautifully engraved, so accurately constructed, and on such a scale. The Danish Government pride themselves on this national survey and its results; and I may add, pride themselves justly. Professor Gunnlaugsson, I believe, spent twelve years in exploring every part of the island that was at all accessible to human footsteps. Every precaution was taken to ensure accuracy. The most minute details were laid down in the map in question, which is indeed the only accurate and trustworthy map—*the standard map*—*of Iceland.*

eruptions in Iceland. Our first inquiry in many instances is, or ought to be, How far are such statements true, or likely to be true? And if we institute such inquiries as regards the histories of the former eruptions of Kötlugjá, we will be forced to the conclusion that almost *none* of the narratives can be accepted as giving an implicitly accurate account of the real phenomena!

Simply as a contribution to the history of volcanic phenomena in a country which, in respect of the importance and interest of such phenomena, is unparalleled in the world, I would have endeavoured to collect into one paper the information—not very accessible to the British geologist—contained in the Icelandic newspapers or other annals regarding the recent eruption of Kötlugjá. In no part of the world are volcanic phenomena on so gigantic a scale as in Iceland. The calculations of Professor Bischoff show that the mass of lava thrown up by the eruption of Skaptar-jökul in 1783 was greater in bulk than Mont Blanc.* The same eruption caused the destruction of some 9336 human beings, 28,000 horses, 11,461 head of cattle, and 190,488 sheep, according to Henderson,† either 1. Directly by the molten lava; 2. The noxious sulphureous or other vapours causing pestilences; 3. The desertion of the coasts by fish; or, 4. The devastation of the pasturages by ashes and sand. In Iceland there are lava streams 50 miles long, 12–15 miles broad, and 600 feet deep.‡ Portions of these streams sometimes form hills as high as Arthur Seat or Salisbury Crags; and such is the persistence of the heat, that rents in the lava have been found still smoking or filled with hot water so long as eleven years after an eruption. In no part of the world, of the same extent, are there so many widely separate vents or foci (about twenty) of subterranean igneous action. The boiling springs, which are most numerous, show of themselves that such action is going on *under the whole island.* But I am the more inclined to regard an account of the recent eruption of Kötlugjá as an interesting, though very small, increment to geological knowledge, from the conviction, to which I have been led by studying the literature of Icelandic volcanoes in the library of the University and other public institutions, in Edinburgh, that comparatively little is known in this country regarding any of these volcanoes with the single exception of Hekla. I have frequently, indeed, met with the belief that Hekla is either the only, or the principal, volcano in Iceland. Both these suppositions are erroneous. I have already casually stated that there are probably twenty separate volcanoes or volcanic vents in Iceland, and Hekla is not the most important of these from any point of view. In regard to the extent of its eruptions, the quantity of lava thrown out, or the devastation caused to men and cattle, it falls very far short of Skaptar-jökul. In point of geological interest, it is inferior to the volcanoes around the Myvatn; and it appeared to me less majestic and of inferior elevation to several other volcanoes or jökuls I saw—and I had the opportunity of seeing only a few of those of the south and west coasts. "There is little in the appearance of Hekla to attract the notice of the traveller," says Henderson, vol. i. p. 341, "even supposing him never to have seen any other mountains but those in the vicinity. . . . When I had it direct before me at a distance of about twenty-four miles, it sank into comparative insignificance; nor do I conceive there is anything about Hekla that is calculated to make an indelible impression on the memory except an actual eruption." . . . Hekla's reputation, I fear, is destined, by the gradually becoming more frequent visits of British tourists and naturalists, to pale and fade, while that of Skaptar, Krabla, and Kötlugjà, will probably increase correspondingly. Next to Skaptar-jökul, Kötlugjá is apparently the most dreaded of the Icelandic volcanoes, and is the most interesting and important in regard both to the character of its phenomena and to the devastations which its eruptions have from time to time produced. According to some writers, the first eruption of Kötlugjá in 894—the last being the fifteenth—is the first volcanic eruption in Iceland chronicled in Icelandic annals, a circumstance which, if true, ought to give to this volcano an additional claim to our attention.*

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* A larger mass of lava by far than was ever thrown out from a single volcano at one time in any part of the world.

† Vol. i., p. 275.

‡ Ibid., vol. i. p. 287.

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* The Edinburgh Cabinet Library volume on Iceland (p. 361), however, mentions the earliest recorded volcanic eruption—in the 9th century—as that of Eldborg, a mountain at the base of the promontory of Snæfellness in the Snæfell-syssel.

So little is known in this country regarding Kötlugjá that I need offer no excuse for prefixing a few remarks on its topography and geology. In the title of this paper, and throughout these remarks, I speak of Kötlugjá as a *volcano*. In doing so, I follow the Icelanders themselves, and I conform to usage; but the term is one of convenience, and is used in its widest sense, as signifying a mere vent for the ejection of igneous matter from the interior of the earth. Kötlugjá* is not a volcano of typical or usual form—that is, an isolated cone-shaped hill, with an apical, distinct, single crater. The termination of the word Kötlu*gjá* explains its true character. The word or affix *gjá*, in Icelandic, signifies a fissure or chasm in the earth, produced by a subterranean igneous action of the same character as that which gives rise to earthquakes. Such fissures are not quite peculiar to Iceland; but in no volcanic country are they so numerous, so characteristic of the scenery, or on so large a scale. They are not common on the large scale in the well-known volcanic districts of the south of Europe—viz., those of Naples, Sicily, or Ischia. But I have lately seen them referred to as occurring in connection with other volcanic phenomena in Syria and Peru. Mr Hogg mentions deep chasms or fissures, resembling the *gjás* of Iceland, as occurring in Lejah and Safah in Syria.* "Late news from Peru," says a writer in Chambers' Journal, "has attracted the attention of geologists, because of the terrible earthquake which has there taken place, with the *unusual* phenomenon of large chasms left permanently open at the foot of the mountains."† In Iceland, these *gjás* are of enormous size, both as to length, breadth, and depth. I do not wish to enter into detail, but would only refer to the descriptions by travellers of the well-known *Allmannagjá*‡ and *Hrafnagjá*, near the Lake of Thingvalla, on the road between Reykjavik and the Geysers. Many extensive *gjás* occur also in the Reykianes district in the south-west, and in the Myvatn district in the north-east. The fissure called Kötlugjá is so large as to resemble an extensive valley; it is situated on the north-east shoulder of Myrdals-jökul, and its direction is first from S.W. to N.E., and then turning at a right angle from S.E. to N.W. There is no crater§ distinct from this fissure, which may be considered as either itself the crater, or as containing one or more apertures of eruption. Nothing, however, is known of the interior of this fissure, which has only been seen from a distance by one or two favoured and fortunate individuals. "Olafssen and Povelsen proceeded within a short distance of it in 1756; but were obliged to give up their attempt, as they were enveloped in snow and mist and exposed to the rage of the volcano, which had been seen to emit flames only two days before."* Henderson saw the fissure from a mountain behind Skaptafell, slightly to the north-west of Oræfa-jökul, and he speaks of its "tremendously yawning crater" as "distinctly visible" at that distance, apparently about sixty-five miles in a direct line (vol. i. p. 264). A clergyman, of the name of Jón Austmann, who, with a guide, ascended Kötlugjá on 12th August 1823,† has put on record a narrative of his excursion. He too only saw the fissure from a distance; he describes it as quite inaccessible, having proceeded towards it till all progress was stopped by enormous walls of basalt and obsidian. Radiating from the primary fissure or *gjá*, as from a centre, diverge in every direction smaller chasms or rents, the depth of which Mr Austmann estimated at several hundred feet. The latter are usually filled or bridged over with ice or snow, and are therefore sources of peculiar danger to the traveller. Such dangers and difficulties in ascent are by no means peculiar to Kötlugjá; they have been encountered on Snæfell-jökul, Oræfa-jökul,‡ and others of the Icelandic jökuls—one range of which, from its inaccessibility, having never yet been explored.§ As a general rule, no competent guides are to be had; and even if they were, this would

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* Kötlugjá is very variously spelled, referred to, or laid down in works of travel and maps. It is sometimes called simply Katla, Kotla, or Kötlu; at other times the word "jökul" is added, and it becomes Katla, Kotla, or Kötlu jökul. In some maps it does not appear at all, either as fissure or volcano. This is the case in the map of Iceland attached to that of Denmark in Keith Johnston's "National Atlas," which is generally supposed to be the most accurate and beautifully drawn atlas extant. This map of Iceland is less inaccurate than former British maps of that island—not one of which, however, is in all respects correct. Indeed, the only accurate map of Iceland as yet published in any country (and it still leaves some 400 square miles of snow-field unaccounted for) is that of Gunnlaugsson before mentioned; a map which does infinite credit both to its author as an individual, to Iceland, and to Denmark. The same map, on a smaller scale than that mentioned in the foot-note of page 10, may be had at a correspondingly less cost; and by travellers it is generally preferred to the larger one, as more convenient, while it is equally accurate. In the map contained in Henderson's "Iceland," Myrdals-jökul is called Kötlugjá-jökul; but the whole map is so inaccurate that it cannot be relied on. Arrowsmith, in a chart of Iceland published in 1808, makes a mistake of no less than sixty-seven miles in the longitude of the east coast, as is pointed out by Barrow ("A Visit to Iceland by way of Tronjem in the Flower of Yarrow yacht in the Summer of 1834." Lond. 1835, p. 82). And in still older maps, both Danish and English, the name of Reykjavik, the present capital of Iceland, does not appear—the capital being therein stated to be Skalholt, which is now a mere farm! "Great inaccuracy prevails in the old maps regarding the position of Iceland, most of them . . . . placing the North Cape in far too high a latitude. According to Olafssen's map, its extent would be 56,000 square miles. . . . Egger's reduces it to 29,838." So says the author of "An Historical and Descriptive Account of Iceland, Greenland, and the Faröe Islands, with Illustrations of their Natural History," being volume twenty-eight of the Edinburgh Cabinet Library: Edinburgh, 1840 (footnote p. 19). This volume is an excellent compilation of all that was known regarding Iceland up to the date of its publication; and even yet, in many respects, it is by far the most serviceable handbook for the traveller, seeing that the ubiquitous "Murray" has not yet sent special envoys to Iceland. But in certain other respects it is not altogether to be trusted; in the chapters on Natural History, for instance, it appears to me to contain many errors, of which more anon. These, however, are not due to any inherent deficiency or inaccuracy in the work as at the date of its publication, but are to be attributed simply to the progress that has since been made by the explorations of geologists, botanists, and zoologists in different parts of Iceland.

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* "On Gabel Hanrân, its adjacent Districts and the Eastern Desert of Syria; with remarks on their Geography and Geology." By John Hogg, M.A., F.R.S., &c.; Edinburgh New Philosophical Journal, April 1860, p. 189.

† Chambers' Journal, June 30, 1860, p. 416: "Science and Art of the Month."

‡ Henderson, vol. i. p. 32, sets this down as "not less than 180 feet deep; in many places nearly of the same width; and about three miles in length." And at p. 35, speaking of the same neighbourhood, he describes "two parallel fissures, which in most places are upward of forty fathoms in depth, and in some places *no bottom can be found at all.*"

§ The prevalent idea of a volcanic *crater* is, that it is a cavity of the shape of an inverted cone, perched on the summit of an isolated mountain. This is fre-

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quently the case; but, in Iceland especially, craters are exceptional and most irregular in their character. They are apertures of every form and size, appearing indiscriminately on all parts of *valleys* as well as hills.

* Henderson's Iceland, vol. i. p. 310.

† The eruption of 1823 terminated, according to some accounts, on 18th, to others, on 26th, July; so that this excursion was made within a few weeks after an eruption.

‡ A graphic account of Dr Pálsson's ascent of this jökul will be found in Henderson, vol. i. p. 248.

§ I allude to the Klofa-jökul in the south of Iceland, now the only unexplored part of the island; which, according to Henderson (Introduction, viii. vol. i.), not greatly diminish the danger, which consists chiefly in the fissures in the earth—filled or bridged over with ice or snow, as I have described in regard to Kötlugjá—crevices in the ice or snow-fields, and immense walls of igneous rocks, much too precipitous to scale. Add to this, the usual absence of harbours of refuge, or places of refreshment of any kind; the impossibility of procuring pasturage for horses, and the consequent necessity of making toilsome pedestrian excursions; and I question whether any mountains in the world, of equal height, are so dangerous in ascent as the jökuls of Iceland. There is thus reasonable excuse for the paucity of information on the physical geography and geology of Kötlugjá and other Icelandic volcanoes.*

Kötlugjá, then, is simply a fissure on the north-east shoulder of Myrdals-jökul, the result of early subterranean disturbances. Myrdals-jökul, again, can scarcely be considered a separate mountain, but is rather part of a range which includes at least Eyafjalla and Godaland-jökuls, according to Gunnlaugsson's map, which, as I have already stated, I regard *the standard map of Iceland.* The height of Eyafjalla-jökul is 5432 Danish feet; the Danish foot being equal to 12⅜th English inches, that is very nearly to an English foot. Myrdals-jökul is somewhat less lofty. Eyafjalla-jökul is a separate centre or vent of volcanic eruption. In the narrative of the earlier eruptions of Kötlugjá, given in the "Islendingur" of 16th June 1860 (No. 6), an eruption of Eyafjalla in 1821†

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occupies a space of not less than 3000 square miles! More modern authors estimate this unexplored territory, covered by perennial snows, at 400 square miles, and I am inclined to regard this as the more correct.

* If the young undergraduates of Oxford and Cambridge—whose excessive muscular activity, youthful enthusiasm, and English love of peril and adventure annually impel them in dozens to risk limb or life on the Alps, which have been overdone and overrun by British tourists—would betake themselves instead to the jökuls of Iceland, they might manage the trip at equal, perhaps less, expense; they would reap the pleasures of novelty; they would encounter equal perils, and they might earn nobler laurels, inasmuch as they could do much to elucidate the physical geography and geology of one of the most wonderful and interesting countries in the world.

† It occurred on the 19th December; was characterised by the ejection of much sand, but did little damage. The eruption is reported in the "Klaus-

is included; but in Dr Hjaltalin's chronological account it is omitted, and we prefer to follow his example. Myrdals-jökul, according to Gunnlaugsson, is situated in 63° 40′ north latitude, and in 19° 40′ west longitude; and Kötlugjá is a little to the south of the one parallel and to the east of the other—about twenty miles inland from the south coast of the island. The range of jökuls which I have just described is frequently the first land on the coast of Iceland sighted by vessels coming from Faröe or Scotland. It has a most imposing appearance in fine weather; and, with the exception, perhaps, of Snæfell on the west coast, and Oræfa-jökul on the south coast more to the east, they are among the finest mountains to be seen on the Icelandic coast. Eyafjalla, Godaland, and Myrdals-jökuls, form a range about forty miles long, say some accounts; fifteen to twenty says Dr Hjaltalin, and twenty to twenty-four broad, running in a direction nearly E. and W.

Geologically speaking, Kötlugjá consists of *Palagonite-tuff*, which is intersected here and there by trap-dikes of newer formation, mostly of basalt or obsidian. Though not quite peculiar to Iceland, for it occurs likewise in Sicily, yet palagonite-tuff is so largely developed in Iceland, is of such interest as the oldest geological formation therein, and is comparatively so little known to British geologists, that I need offer no apology for devoting to it a few descriptive remarks. The mineral *palagonite* was discovered in 1838 as the basis of the volcanic tuff of Aci Castello in Sicily, by W. Sartorius Von Waltershausen.* He was led to give it this name from having found it in a volcanic tuff very abundant in the neighbourhood of Palagonia, Sicily, during an excursion to the celebrated Val di Nota in the autumn of 1840. The similarity of this tuff to the black basalt tuff of Militello, has been

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turpost," a former Icelandic newspaper, not now existing. According to the Edinburgh Cabinet Library volume on Iceland, the eruption began on 20th December, and continued with intermissions till July 1823, during which latter month its chief or only ejection consisted of streams of water. When this eruption ceased, the 14th eruption (hereinafter recorded) of Kötlugjá began.

* Physisch-Geographische Skizze von Island mit besonderer Rücksicht auf Vulkanische Erscheinungen: abgedruckt aus der Göttinger Studien: Göttingen, 1847." A small brochure, costing in this country about 2s., and containing by far the best and most recent account of the geology and physical geography of Iceland.

proved by the analyses of Dr Merklein.* Lyell describes palagonite as "rather a mineral substance than a mineral, as it is always amorphous, and has never been found crystallized. Its composition is variable; but it may be defined as a hydro-silicate of alumina, containing oxide of iron, lime, magnesia, and *some alkali*. It is of a brown or blackish-brown colour, and its specific density 2·43."† A specimen, labelled *palagonite*, from the valley of Seljadalur, between Reykjavik and the Lake of Thingvalla, sent me by Professor Hannes Arnesen, lecturer on Natural History in the Government College, Reykjavik,‡ is a substance apparently of a very composite character. To the naked eye it is of a blackish-brown colour, is amorphous and compact, and of a semi-vitreous lustre, somewhat resembling a pitchstone. It feels heavy and massive. Under the lens, it is seen to vary both in colour, texture, and density. Some portions are compact and glassy, resembling obsidian; others are spongy, porous, or slaggy, resembling pumice or slaggy-lava. Certain portions, which are generally also spongy or porous, are of a leek-green colour, and appear to be olivinic débris; others are pitchy black, and at the same time compact and vitreous, which are apparently augitic or hornblendic débris. Other portions, again, have more of a red colour, and resemble the compact Colophonite of Arendal, or the compact titaniferous Pyrope of Christiansand, both in Norway; such portions seem the débris of garnetic minerals. Lastly, there are included what appear to be fragments of jasper, hornstone, and chalcedony, or the débris thereof. What has been sent me by Professor Arnesen as *palagonite-tuff* from the same locality, appears to be essentially the same thing in another form; it is more porous and friable, more intermixed with zeolites, or with small impacted fragments of obsidian-like character; more of a red tinge, or garnetic; and in some places more of a green tinge, or olivinic. These specimens are widely different from the palagonite-tuff collected by myself on the coast midway between Reykjavik and Havnafiord, or sent me by Dr Hjaltalin from the Fossvogur near Reykjavik. In the former place it is stratified and fissile, the rock forming low cliffs, which are rapidly being eroded by the waves. Some specimens were of the ordinary colour of clay, grayish, with a tendency towards a brownish rather than a bluish tint. Others were decidedly of a brown tint, while a few were ochreous from impregnation with peroxide of iron. The Fossvogur specimens are more of a brown tint, more granular and arenaceous, and evidently contain more iron than those collected by myself. The iron shows itself on weathered surfaces and in fissures, frequently as a coating of pavonine lustre or aspect. All the palagonite rocks are more or less ferruginous. *Palagonite lava*, from the Vatn, Reykjavik, is of so deep a brick-red colour that it resembles an iron slag, were it not for its superior lightness. To the naked eye the specimens collected by myself have the appearance of ordinary slate clays, but under the lens the following peculiarities are observable. In some specimens there are fragments of the shells of marine molluscs, beautifully preserved, but they are generally very few in number. Interspersed through the argillaceous basis are numerous masses, from the size of a pea to that of a hazel-nut, of a spongiform texture, most frequently of a leek-green colour and olivinic character, sometimes black and obsidian-like. There are also minute crystalline masses, frequently about the size of a pin's head, some white, others black, in both cases generally of a vitreous lustre, and with apparently a conchoidal fracture; and, lastly, there is occasionally much intermixed granular matter of an olivinic aspect. These contents of the tuff are, it will be at once perceived, similar to, if they are not identical with, those of the palagonite and palagonite-tuff of Seljadalur. Two series of the palagonite-tuffs collected by myself were recently kindly analysed for me by Dr Murray Thomson,

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* In the "Göttinger Studien," 1845. Part I. p. 402.

† Manual of Elementary Geology. By Sir Charles Lyell, M.A., F.R.S. 5th edition. Lond. 1855. P. 474.; and Professor Bunsen of Heidelberg (who has recently been awarded the Copley medal of the Royal Society of London for his chemical researches) on Palagonite in the "Annalen der Chimie und Pharmacie," lxi. 268.

‡ In addition to the rocks and minerals collected by myself while in Iceland, I have been fortunate enough to receive from Professor Arnesen and Dr Hjaltalin a portion of the duplicates of the geological and mineralogical collection in the Government College of Reykjavik. Both these gentlemen are excellent geologists and mineralogists, and I therefore feel warranted in relying on the authenticity of the names and localities appended to the specimens sent me.

Analytical Chemist, and Lecturer on Chemistry in the Royal College of Surgeons Medical School, Edinburgh. No. 1 contained silica, alumina, lime, and iron; while No. 2 contained 8·03 per cent. of iron.* The former was of a grayish colour,

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* Though not illustrative directly of the structure or phenomena of Kötlugjá, still, as contributions to the natural history of volcanic products in Iceland, I may here take the opportunity of recording some further analyses, kindly undertaken for me by the same chemist, of various hot-spring waters and deposits, volcanic rocks, &c., collected by myself in Iceland last summer. I have only to premise that the phrases *abundant*, *traces*, &c., used by Dr Murray Thomson in the following analyses, are intended to give an idea of the *quantity*, where the amounts are not of sufficient interest or consequence to be more precisely noted.

I. Water from the Hot-spring of Laugarnes, about three miles to the north-east of Reykjavik.

Total solid matter in a pint, ascertained by experiment = 3·51 grs.; sp. gr. at 60° = 1000·21, water being 1000.

Silica,	1·04* grains.
Protoxide of iron,	·24
Lime,	traces.
Magnesia,	·02
Soda	·84
Sulphuric acid,	·76
Chlorine,	·40
Organic matter,	·30
	3·60

II. Various muds† or other deposits from the same spring.

No. 1.	Cobalt,	traces.
	Manganese,	abundant.
	Iron,	do.
	Lime,	very abundant.
	Magnesia,	do.
	Soda,	in marked quantity.
	Silica,	abundant,
	Sulphuric acid,	in considerable quantity.

No. 2. Silica, alumina, iron, manganese, magnesia, and lime.

No. 3. Silica, iron, alumina; trace of manganese, lime, and magnesia.

No. 4. Silica, iron, alumina, and magnesia, but neither lime nor manganese.

III. Mud†-deposits from the Hot-springs of Krisuvik in the south-west of Iceland.

No. 1.	Iron,	large.
	Lime,	do.

* This large amount of silica is confirmatory of all former published analyses of the waters of the Geysers and other hot-springs in Iceland.

† In the various mud-deposits, Dr Thomson made careful but unsuccessful search after several of the rarer earths, as yttria, glucina, and oxide of cerium.

and clayey aspect; the latter had an ochrey character. It will be observed that the composition of palagonite-tuff, according to the analyses of Dr Thomson, nearly corresponds with that of the isolated mineral palagonite, according to the analyses of Professor Bunsen, as quoted by Lyell. Dr Thomson, however, found no magnesia in my specimens; and this is nowise remarkable, when the striking differences in the character of palagonite-tuffs is borne in mind, differences which probably imply corresponding variations in chemical composition. The "some alkali" of Bunsen is too vague an expression and finding to be of any use as an item of comparison. My specimens of palagonite-tuff, then, may in general terms be

Magnesia, . , . .	small.
Soda, ...	moderate.
Sulphuric acid, . . .	large.
Silica, ...	do.

The latter, when separated, beautifully white and pure.

No. 2. Mud of a bluish colour.

Silica, iron, alumina, manganese, lime, sulphuric acid (partly in a free state), phosphoric acid in small quantity.

IV. Crude Sulphur or Sulphur-muds from the Sulphur-beds of Krisuvik.

1st determination gave .	98·20	per cent. sulphur.
2d do. .	96·39	do.
Mean of the two analyses,	97·29	do.*

V. Havnafiordite† of Forchhammer, from the hills immediately behind the village of Havnafiord, eight miles south-west of Reykjavik, Iceland.

Silica, ...	35·89
Lime, ...	10·86
Sulphuric acid, . . .	1·55
Protoxide of Iron, . . .	14·41
Alumina, ...	27·36
Potash, ...	9·00
	99·07

VI. Trachyte (coloured green apparently by chlorite), from the Island of Videy, Bay of Reykjavik, Iceland.

Silica, alumina, iron, traces of Magnesia.

VII. Limestone (in boulders) Thorshavn, Faröe.

68·12 per cent. carbonate of lime.

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* This is an unusually high percentage of sulphur, the crude Sicilian sulphurs usually containing not more than 80 to 90 per cent.

† The sp. gr. is usually 2·729; it appears to be a Lime-oligoclase, belonging, therefore, to the Felspathic family of minerals.

described as virtually siliceous slaty clays, frequently calcareous, sometimes to such an extent as to become marly; frequently ferruginous, sometimes to such an extent as to become ochrey; frequently granular, and to such an extent as to resemble argillaceous sandstones. Von Waltershausen describes the Icelandic palagonite tuffs as abounding less or more either in marine shells or in the siliceous skeletons of Infusoria.* The Sicilian palagonite-tuff is similarly characterised by the presence of marine shells; in the basalt-tuff of Militello, there are about ninety species of the shells of mollusca, or of the hard parts of echinoderms and crustacea. If the lime,† which palagonite-tuff contains, is not entirely derived from such shells, there is every reason to suppose that it is in a great measure so, and that the percentage of lime must depend on the percentage of shells. The number of shells and the proportion of lime are sometimes so great as to give the tuff the character of a clay-marl, and as such it might be used with advantage in Icelandic agriculture,* were there such a thing. Again, the percentage of silica may depend in great measure on the quantity of included infusorial skeletons.

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* This infusorial character of volcanic tuffs is not peculiar to Iceland. Sir Charles Lyell (in his Principles of Geology, p. 388, 9th ed., Lond. 1853) gives many other instances. With the exception of the Patagonian pumiceous tuffs, which contain *marine* Infusoria, all the tuffs he mentions contain *fresh-water* Infusoria. Such tuffs cover Pompeii; they are common on the Rhine, *e.g.*, at Hochsimmer, on the left bank, near the Laacher see, and in the Brohl valley; and they are to be met with also in Mexico, Peru, and the Isle of France. It is supposed that the materials composing some of these infusorial beds fell as dust, while that of others issued as mud from craters, which were in some instances submarine. Professor Bunsen regards the presence of Infusoria in Icelandic palagonite-tuff as a proof of the formation of the latter in thermal waters. Professor Quekett and Dr Gulliver of London, two of the most experienced and eminent microscopists in Britain, have kindly examined for me specimens of *all* the Palagonites and Palagonite-tuffs mentioned on pages 19 and 20 of this paper. But, in *none of them*, even after boiling in nitric acid, did they find even traces of Infusoria or their skeletons!

† While in Iceland I was asked by a Dane to see with and for him an Icelandic rock, which he thought, if pulverised, would make an excellent mortar or cement. Under this impression, he had visions of opening a quarry and making his fortune by supplying it to Denmark, as well as to Iceland itself. The rock proved to be this palagonite-tuff, and the site of his visionary quarry the very spot where I had collected my own specimens of the said tuff. I was sorry to be the means of demolishing all his "castles in the air," by expressing my conviction that the rock in question would be useless for the purpose specified, from its small percentage of lime. Specimens of Palagonite-tuff from the same locality have recently been examined chemically by Dr Murray Thomson, specially with a view to ascertain whether it might become at all serviceable as a cement or mortar. His experiments all go to prove the reverse, and therefore to support the opinion I originally hazarded.

Palagonite occurs as a conglomerate, as well as a stratified fissile rock, resembling slate clay; and there is every gradation between them. The conglomerate sometimes forms huge irregular rock-masses, enclosing fragments of amygdaloidal or other trappean rocks. The stratified fissile rock is sometimes so granular and arenaceous as to be mistaken for sandstone. It is generally supposed that these tuffs were originally the ashes, sand, and mud, thrown out by submarine volcanoes—formed of the débris of felspar, augite, olivine, magnetic iron, peroxide iron, or of the other constituents of trap rocks—assorted under the influence of water, time, high pressure, high temperature, and the chemical action of sea-water, on the bed of the sea, the varied materials being partly united mechanically, partly chemically. The aqueous origin of palagonite-tuff is supposed to be proved by—(1.) Its stratification; (2.) The presence of marine shells, or of the skeletons of Infusoria; (3.) The fact that, in volcanic stratified tuffs formed on land, palagonite never occurs; and (4.) The circumstance that the latter mineral contains 17 per cent. of water. The superior hotness of the sea-water, in which palagonite-tuff was formed, to that of the sea-water now washing the coasts of Iceland, is believed to have been due to the contiguity of the submarine volcanoes, of the products of whose eruptions such tuff itself was formed; and the supposition of such superior hotness is moreover held to be borne out by the included shells having preserved their fresh appearance and colours, and by the presence of Infusoria. High pressure, again, is presumed to have had considerable influence, from the consolidation of the tuffs, and from the crystalline character of their contents. Such are the views of Von Waltershausen and Bunsen, and other geologists and chemists, who have the most thoroughly studied the structure and mode of formation of these tuffs, though they are somewhat at issue in various details of their beliefs.

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* "Agriculture," says the Edinburgh Cabinet Library volume on Iceland, "cannot be said to exist" (p. 205). There appeared to me much land in Iceland that might, with British enterprise, be made available in agriculture. Henderson is evidently of the same opinion, for he remarks, vol. ii. p. 129, "Many of the plains . . . are suffered to lie waste, but would furnish excellent farm-lands were they to undergo cultivation." I am aware that at least one Scotch merchant some time ago offered to demonstrate to the Icelanders the results which could be brought about by Scotch farming on their barren soil, if they would only lease or sell to him some then and still useless ground at a reasonable figure. The prejudices and power of the Danish merchants, and the apathy of the Icelanders, combined to cause his offer to be rejected, greatly to their own loss, I opine. The Icelanders blame their rulers, the Danes, as the obstacles to all progress. How far this may be true I cannot pretend to decide; but certain it is that I failed to find any *proof*, satisfactory to my mind, of the justice of any such asseveration. There would appear to be a something in the constitution of the Icelandic mind which is opposed to innovations—in regard at least to agriculture and social economy. What that something, however, is, this is not the place to inquire.

They agree, however, that the palagonite-tuff is the oldest formation in Iceland—the geological basis, in fact, of the island, through which the trachytes and traps have subsequently burst.* It plays a much more important rôle in the geology of Iceland than in that of Sicily; in the former island it forms not only whole mountains and ranges of mountains, but probably also whole belts of country. Von Waltershausen regards it as probable that a belt of it stretches right across the island from S.W. to N.E., or, in other words, from Cape Reykianes to the Myvatn, including the chief volcanoes of Iceland. In Keith Johnston's map of the volcanoes of Iceland (in his "Physical Atlas,"†), and also in Map IV. attached to Daubeny's work on Volcanoes, such a belt is laid down as of *Trachyte*. But I am disposed to agree with Von Waltershausen, and to think that it is more likely to be of palagonite-tuff. However this may be, there seems no doubt that this tuff constitutes the cone-shaped jagged mountains which occupy the greater part of the Guldbringésyssel, which are a prominent feature of the landscape, looking from Reykjavik southwards in the direction of Krisuvik, and which Sir George Mackenzie* calls the "Sulphur Mountains." It also forms the whole range of the southern volcanoes: Hekla, where the strata are much elevated and tilted up; Thrihyrningr, Tindfjalla-jökul, and Eyafjalla, as well as the mountains which the Icelanders call "Moberg."

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* This view of Bunsen and Von Waltershausen is, however, criticised in an interesting "Contribution to the geognosy of Iceland, from observations made in the summer of 1850," by Theodor Kjerulf, p. 54 (Bidrag til Islands geognostische Fremstilling efter Optegnelser fra Sommeren 1850;" published in the "Nyt Magazin for Naturvidenskberne," vol. vii. part 1, Christiania, 1853; an excellent equivalent to our "Annals of Natural History," or, translated more literally, "New Magazine of the Natural Sciences," which records the transactions of the Physiographical Society of Christiania.)

† Map No. 7 of those illustrative of *Geology*, which is mainly founded on the various publications of Krug von Nidda; "Memoirs on the Geology of Iceland," &c., in Karsten's "Archiv für Mineralogie," vols. vii. xi. &c., and translated in Professor Jameson's Philosophical Journal, vol. xxii., &c.

I have had no opportunity of examining the basalts and obsidians of Kötlugjá; but, as a general rule, the former resemble those of Scotland, while the latter, from some localities—*e.g.*, in specimens I have from Hekla—are scarcely distinguishable from the Arran pitchstones. In basalt from the Laxá elv or Salmon river, near Reykjavik, the base is more of a dull earthy aspect than is common in our basalts. The rock is very black and very heavy; it contains large quantities of a black glassy mineral, sometimes slightly porous or slaggy, resembling obsidian, having a conchoidal fracture, and which may be a form of augite. There are also crystalline masses apparently of olivine, of a dirty yellowish-green colour—this colour resulting probably from partial weathering. In the Island of Videy, I found a vitrified, very heavy basalt, occurring in narrow veins in basalt having more the ordinary character. The percentage of metallic iron in two specimens of this vitrified basalt was, according to the analyses of Dr Murray Thomson, 7·02 and 13·68 respectively. All the Icelandic basalts,† like our own, seem to be more or less ferruginous—the iron frequently appearing as peroxide on weathered surfaces, or giving a rust-red tint to the mass of the rock. The Icelandic obsidians, though sometimes bottle-green in colour, comparatively opaque at the edges, and otherwise resembling the Arran pitchstones, are generally of a pitchy-black, with a more vitreous lustre and fracture. I saw some fine and very large specimens in the Mineralogical Collection of the Government College at Reykjavik; but I am told still finer were brought from the Myvatn and Krabla district by some of my fellow-passengers in the "Arcturus." Some of the obsidians sent me from Iceland are porphyritic or amygdaloidal, containing crystals of glassy felspar or minute nodules of what appear to be zeolites. A substance, called by Professor Arnesen and Dr Hjaltalin "*Pumice-tuff*," enters—to what extent I know not—into the composition of some of the outlying hills around Kötlugjá. It is of a leek-green colour, is light, porous, and very friable, and consists of a basis of a greenish fibrous mineral, intermixed with the débris apparently of augite, olivine, felspar, zeolites, and other minerals common in trappean rocks. The mineral basis more resembles tremolite or actinolite than pumice or asbestos; the colour is green, and it appears made up of aggregated needle-formed

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* Travels in the Island of Iceland during the Summer of the year 1810. By Sir George S. Mackenzie, Bart., F.R.S., &c. Edin. 1811: containing a Preliminary Dissertation on the History and Literature of Iceland, as well as sections on—(1.) Present State of Education and Literature; (2.) Government, Laws, and Religion; (3.) Diseases of the Icelanders—all by Sir Henry Holland, M.D.: and a section on Zoology and Botany, by the late Dr Bright.

† "True basalts," says the Edinburgh Cabinet Library volume on Iceland, p. 348, "in which the minerals are so intimately blended that the whole appears as one homogeneous mass, are *nowhere found in Iceland*." This statement is based on the nomenclature of Krug von Nidda. But, in the first place, it is incorrect in fact, inasmuch as the Videy basalt, collected *in situ* by

myself, is as fine in texture as that of the summit of Arthur Seat, which it otherwise resembles. In the second place, on the one hand, the "*excluding* from the basalts all rocks, in which the constituents can be discerned by the eye," and, on the other, *including* "those, which possess the columnar or so-called basaltic structure" (*Ibid.* p. 349, foot-note), seem to me to be unnecessary refinements in geological classification and nomenclature, seeing that basalt and greenstone are but different names for different conditions of what is to all intents and purposes *the same rock*. Basalt and greenstone pass into each other by gradations so nice that it is impossible to define where the one ends and the other begins. The difficulty of definition is sometimes in a measure avoided by the use of such terms as basaltic greenstone or greenstone basalt; but these terms only indicate that the texture is becoming fine in the one case and coarse in the other! The author of the same volume on Iceland appears to commit further mineralogical or geological errors (as I may elsewhere show he also does botanical ones), as, for instance, where he says (p. 365) or implies that Olivine is absent from the Icelandic rocks. I have found this mineral not only most abundant, but in a pure form, and in large and conspicuous crystals or crystalline masses, in the lava of Havnafiord. It also seems to occur in an altered form in the basalt of the bed of the Laxa [river] near Reykjavik, exactly as in some of the porphyritic basalts of Arthur Seat; and in a still more altered form in other Icelandic rocks, as I have already casually mentioned when speaking of the Palagonite series.

crystals, rather than of silky threads.* Altogether, it seems to belong to the hornblendic rather than to the felspathic family.

The physical geography of the lower or level country around the range of jökuls in which Kötlugjá is situated, is sufficiently indicated on the appended map. (Plate II.) It is constructed from the details given in Professor Gunnlaugsson's large map of Iceland formerly referred to. But Dr M'Kindlay of

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* Also closely resembling this mineral basis, it may not be too irrelevant here to mention—is a variety of Quartz, occasionally occurring in trap-rocks, and which is pseudo-fibrous, stellate-radiate, and coloured green by chlorite. I have lately met with abundant and beautiful specimens of it, associated with ordinary Quartz and with Calcareous and Heavy Spars, in cavities or veins in the greenstone of Corsiehill quarry, on the north slope of Kinnoull Hill, Perth. It occurs chiefly in the form of small nodular masses, imbedded in a granular or earthy chlorite; the said masses being generally about the diameter of a shilling piece; the fibres, or, more correctly speaking, the closely aggregated needle-shaped crystals, radiating outwards from a centre, the crystals being broadest or largest at the periphery; the whole mass coloured of a deep leek-green by chlorite. The order of arrangement of this and the accompanying or associated minerals in a cavity is generally as follows:—Most externally, or immediately lining the greenstone, is a more or less deep layer of earthy chlorite; in this are imbedded, but more internally, the nodules of pseudo-fibrous, stellate-radiate quartz, above described; still more internally, there is a shell of quartz,—green externally from chloritic coloration, and coarsely pseudo-fibrous,—gradually losing its colour and fibrous character as it approaches the centre of the cavity, where it is colourless, and crystallises in the ordinary form, bearing on the surface of its crystalline internal terminations tabular plates of calcareous, or masses of heavy, spar. In different specimens, the fibres or needle-like crystals vary greatly in fineness. When they are very fine and almost silky, like Asbestos or Tremolite, the nodular masses are small, and the mineral resembles Pectolite, for which it was at first taken. When larger and coarser, the masses are correspondingly larger, and the resemblance is then greater to Prehnite. In both cases, the first aspect is Zeolitic. Before examination, therefore, I had doubts, which were shared in by mineralogists and chemists to whom I submitted specimens, as to the true nature of the mineral in question. But there can no longer be any doubt, seeing that the following experienced mineralogists have pronounced that it is simply a variety of Quartz, viz., Mr Maskelyne, Reader in Mineralogy in the University of Oxford, and Superintendent of the Mineralogical Department of the British Museum; Mr Warrington Smyth, Lecturer on Mineralogy and Mining in the Government School of Mines, Jermyn Street, London; Dr Forster Heddle, presently acting as Lecturer on Chemistry in the University of St Andrews; and Mr R. P. Greg of Manchester, joint author with Mr W. G. Lettsom of the recently published excellent "Manual of the Mineralogy of Great Britain and Ireland." The latter gentleman informs me, however, that it is "a very unusual variety."

Glasgow, who visited this district in 1859, informs me of some particulars in regard to which he thinks the latter map requires emendation. He mentions, for instance, that while traversing on horseback the marsh district called Austr-land Eyjar to the west of Eyafjalla, he crossed an extensive belt of black volcanic sand, which does not appear on the map. Between the range of jökuls—which includes Eyafjalla, Myrdals, and Godalands-jökuls—and the sea, to the south, the lowlands consist mostly of marsh land, including meadow land, and to a less extent of sandy or stony wastes, the sand being black volcanic sand. To the north of the range of jökuls, the lowland is mainly heath or moor land, and partly sandy wastes; on the east it is chiefly of the latter character, and to a small extent marsh land; and on the west, partly marsh, partly sand, and partly heath. Generally speaking, the jökuls are bounded on all sides by hills of minor elevation, which are not covered with snow throughout the year. This is especially the case to the north-east of the range; to the north, west, and south of Eyafjalla, and to the south of Myrdals-jökul. Henderson describes Myrdals-sand, to the south-east of Myrdals-jökul, an extensive tract of volcanic sand and ashes, "forming one of the rudest and most forbidding scenes imaginable" (p. 310), as altogether formed of the products of the eruptions of Kötlugjá. The promontory of Hjörleifshöfdi —740 Danish feet high—he also says, has "sides nearly perpendicular, in some places excavated, and to the west and east its base has been terribly scooped by the deluges poured down upon the plain" by the same volcano (p. 309). Henderson further refers to hills or hillocks of gravel, sand, and ice (?)—scattered over the lowlands—as the results of the water-floods from Kötlugjá during its eruptions.* We have therefore sufficient evidence for believing that these eruptions have had a material influence on the physical features of the whole country around Kötlugjá.

As throwing light on the eruption of 1860, it will be desirable to consider the history of previous eruptions of Kötlugjá, which accordingly I will give in chronological order. The volcanoes of Iceland probably stand next to Etna and Vesuvius in regard to the completeness of their chronological records; and I have already mentioned that the record of the first eruption of Kötlugjá in 894 is supposed to be the earliest chronicle extant of volcanic phenomena in Iceland. There is, however, much dubiety and confusion relative to the precise number of eruptions, since the historic era, of particular Icelandic volcanoes; and this fact is well illustrated by the chronology of the eruptions of Kötlugjá since 894. Olafssen and Povelsen* speak of only five eruptions between 894 and 1755, and Sir George Mackenzie does the same. Henderson gives a larger number, and he had ample opportunities for collecting satisfactory information; but his calculation even falls short of that of Dr Hjaltalin, and of the "Islendingur" formerly mentioned.† Inasmuch as Dr Hjaltalin's chronology is the most recent, and is founded on investigations made in the archives contained in the National Library at Reykjavik, I adopt it as likely to be approximatively the most correct. Where Henderson, however, gives an account of the eruptions of Kötlugjá, he is generally fuller than Dr Hjaltalin. The discrepancies between historians as to the precise number of eruptions of Kötlugjá or other Icelandic volcanoes arise from a variety of causes. *Firstly*, The older Sagas or narratives are extremely vague in their topographical references, the localities of eruption occasionally not being mentioned at all! Henderson refers to an eruption in 1717, probably of Eyafjalla, where the volcano is indefinitely termed in the national annals "the Austur-jökul" (p. 328, vol. i.) Further, the descriptive language was so inflated or poetical, that it is now impossible to distinguish what was fact from what was fiction. *Secondly*, The range of jökuls in which Kötlugjá is situated, as well as the individual members thereof, have at different times, and even in different parts of Iceland at the same time, borne different names. The range has been called Eyafjalla-jökul; and while so designated, an eruption of Kötlugjá would consequently rank as an eruption of Eyafjalla. Again, what is laid down on Professor Gunnlaugsson's map as Myrdals-jökul, this name being derived from the parish (Myrdal) on its south side, has at various times been known as Solheima-jökul,* Höfda-jökul and Kötlugjá-jökul. *Thirdly*, Only grand eruptions from the principal or central crater have been apparently recorded by some historians; while minor eruptions from fissures or secondary craters in the sides of the jökuls, or from old lava fields, all distant from the primary crater, have probably either not been observed or recorded. *Fourthly*, On the other hand, where an eruption has been intermittent, and has extended over months, and even, as has been the case, years, the separate outbreaks may have been enumerated as so many distinct eruptions. *Fifthly*, There is the incompetency of the witnesses,† firstly, to observe, and, secondly, to describe, as I have already pointed out (p. 11). Kötlugjá has never been properly explored even during periods of quiescence; the narrators of the phenomena of eruptions have too frequently been terror-stricken, ignorant or superstitious peasants, and all eruptions have been observed only from a greater or less distance—one result of which is, that it can only have been matter of speculation from what precise spot the eruption occurred. For instance, eruptions have been recorded by residents in Reykjavik, eighty or a hundred miles distant, who were not themselves on or near the spot during the said eruptions—who, in fact, have, or had, never visited Kötlugjá or its vicinity at all, and who founded their narratives on the stories of peasants living within perhaps twenty miles of Kötlugjá, and who happened to visit Reykjavik some weeks or months after the date of the eruption to be described. Under such circumstances, it is not unreasonable that we should regard with suspicion, accept under reserve, or at once reject, many of the phenomena recorded as *facts*. I will have occasion to revert briefly to this subject after I have chronicled the fifteen eruptions of Kötlugjá and their phenomena.

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* The Edinburgh Cabinet Library volume on Iceland (p. 363) says, "The débacles of water, too, issuing from the volcanoes, frequently fill the valleys with vast heaps of débris swept down from the mountains."

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* "Vice-Larmand Eggert Olafssens og Land-Physici Biarne Povelsens Reise igjennem Island foranstaltet af Videnskabernes Selskab i Kiobenhavn, 1772;" also translated into German, Leipzig, 1774 and 1775, 4to, 2 vols. One of the earliest, fullest, and most interesting works of travel in Iceland, but containing many assertions and statements that must be taken *cum grano salis*.

† The Edinburgh Cabinet Library volume on Iceland fixes on ten as the number of eruptions between the years 894 and 1823 (p. 361); but it refers two eruptions to Eyafjalla and four to the Solheima-jökul."

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* The Edinburgh Cabinet Library volume on Iceland mentions (p. 361) eruptions of this jökul in or about the years 900, 1245, 1262, and 1717. The second and third of these eruptions are probably those hereinafter recorded as the third and fourth eruptions of Kötlugjá.

† I am speaking in general terms. There have been exceptions. One of the most thorough naturalists that Iceland has ever produced long lived near Kötlugjá, and, as Henderson observes (p. 319, vol. i.), "One would almost suppose he had fixed his residence at this place (*Vik*) for the express purpose of watching the motions of Kötlugjá, as he has only to repair to the summit of the mountain behind his house in order to obtain a magnificent view of the whole region." Unfortunately, however, he had no opportunity of observing any of its grand eruptions, nor of publishing any of his researches on the natural history of Iceland—researches which would most probably have proved of extreme value. (*Vide* Henderson, p. 318, vol i.)

Guided by Dr Hjaltalin's data, I have endeavoured to show concisely in the following table, the eruptions of Kötlugjá, with the intervals between the respective eruptions:—

1st Eruption,	A.D. 894	Interval since previous Eruption.
2d	934	40 years
3d	1245	*311 "
4th	1262	17 "
5th	1311	49 "
6th	1416	105 "
7th	1580	164 "
8th	1612	32 "
9th	1625	13 "
10th	1660	35 "
11th	1721	61 "
12th	1727	†6 "
13th	‡1755	28 "
14th	1823	68 "
15th	1860	37 "

Since the earliest recorded date of any volcanic eruption in Iceland, therefore, there have been fifteen outbreaks of Kötlugjá; or one on an average of every 64·40 years. This average, however, it will at once be seen, on reference to the table, does not give a true idea of the length of the intervals between each eruption. On the contrary, between the second and third eruptions no less than 311 years intervened, though it admits of question whether, during so long a period, there have not been other eruptions, of which we have simply no

* Longest interval. † Shortest interval. ‡ Most important eruption.

records preserved. Again, between the eleventh and twelfth eruptions only six years elapsed. It must further be borne in mind, that the dates given in the above table represent simply the dates of *commencement* of individual eruptions; some of which continued, with intermissions, during one or two years or upwards. The interval between the last eruption of Kötlugjá and that of 1860 is thirty-seven years, and since the last eruption in Iceland, that of Hekla in 1845–46, fourteen years.

1*st Eruption*, A.D 894.—The pasture lands between the hill called Hafrsey and the Holmsá (river) are said to have been destroyed by a formidable eruption of Kötlugjá. Eight farms were abandoned. The district of country in question is still almost entirely a sandy desert. Henderson says the effects of this eruption "are still visible in the tract of ancient *lava* (?) to the east of the mountain" (vol. i. p. 311). This eruption is recorded in the Landnâmabok,* and also by Dean Jón Steingrimsson,† whose MS. is preserved in the Public Library at Reykjavik.

2*d*, 934.—The extensive sandy desert now known as the Solheimasand is said to have resulted from this eruption, which was also apparently a formidable one. This tract is about twenty English miles long, and is formed altogether of volcanic sand, ashes or lapilli, and pumice. The "Islendingur" of 5th June 1860 (p. 39) refers to an eruption in the year 1000, on the testimony of Dean Steingrimsson; but it is altogether omitted, probably as not being properly authenticated, in Dr Hjaltalin's chronicle.

3*d*, 1245.—A tract of country, of what extent we are not informed, was covered with sand and ashes to the depth of 6 to 8 inches by this eruption. But the eruption is said to have proceeded from Myrdals-jökul on the Solheimar side; and though Dr Hjaltalin refers it to Kötlugjá, the topographical accuracy remains doubtful.

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* Islands Landnâmabok, *i.e.* Liber Originum Islandiæ: which contains the earliest annals of Iceland: published at Skalholt, the former capital, in 1688, 4to; also at Copenhagen, 4to, 1774, with a Latin translation, Notes and Indices.

† This clergyman resided for some time at Solheimar in Myrdal, and afterwards at Kirkjubœ in the Sidu district. He wrote about the end of the last century, and has left an excellent account of the celebrated eruption of the Skaptar-jökul in 1783, the most formidable volcanic eruption that has been known during the historic era in any part of the world.

4th, 1262; or, according to some writers, 1263.—The eruption was attended by such an ejection of dust and ashes, that the sun could not be seen at midday in serene weather. During this eruption, the large river called the Fulilækr, or Jōkulsá, which divides the Skogasand from the Solheimasand, suddenly made its appearance. The crater of eruption seems, however, to have been somewhat to the west of Kötlugjá; but as this crater is unknown, and a mere matter of conjecture, the eruption has been usually referred to the volcano just named.

5th, 1311; or, according to some writers, 1332.—There is still greater doubt as to the position of the crater in this eruption, which appears to have been more destructive to life than any of the previous ones. Some writers refer the crater to the Sidu-jökul, others to the Breidamerkr-jökul, while others place it vaguely in the east of the range of jökuls, which includes Eyafjalla and its allies. Many farms were destroyed in the district called Myrdals-sand; several sand hills and other hills were formed, and several marshes sprang into existence. The "Islendingur" account states that this eruption was known as the "*Sturluhlaup*," from only *one man*, of the name of Sturla, having been saved, of those overwhelmed by the volcanic ejections. Henderson's narrative differs somewhat (p. 311, vol. i.): Kötlugjá, he says, vomited "ashes and sand the greater part of the winter (of 1311–12); and, melting the ice about the crater, the inhabited tract in the vicinity was inundated, and all the inhabitants except *two* perished in the flood."

6th, 1416.—This eruption is known as the "*Höfdahlaup*," probably because the lava or water-floods took the direction of Hjörleifshöfdi, an isolated hill and promontory on the coast of the Myrdals-sand, considerably to the south-east of Kötlugjá.

7th, 1580.—During this eruption, it is stated that Myrdalsjökul was "rent asunder;" and as the name Kötlugjá is now first given to the crater or fissure of eruption, it is probable that this is the date, if not of the formation of the chasm called Kötlugjá, at least of the recognition or discovery of its existence. From this date downwards, eruptions from the same quarter of Myrdals-jökul have been referred to Kötlugjá. This eruption was characterised by fire, darkness, and a rain of ashes, as well as by water-floods; one of which latter went eastward towards the Monastery of Thyckvaboe, and another southwards to Myrdal. Many farms were destroyed; but there appears to have been no loss of human life.

8th, 1612.—According to Dean Steingrimsson, this eruption occurred either from Eyafjalla or Myrdals-jökul, or from some point intermediate between them. The accompanying "fire" was such, that the eruption was visible extensively in the north of Iceland. It is conjectured that the eruption was attended by a subsidence to some extent of the Fall-jökul, which is situated between Eyafjalla and Myrdals-jökul, as well as of the lower lands between Langanes and Thorsmerkr.

9th, 1625: one of the grandest and most devastating eruptions of Kötlugjá that has ever occurred. Its historian is Thorsteinn Magnússon, at the time Sysselman (or sheriff) of Skaptafells-syssel (or district),* who lived in the monastery of Thyckvaboe. His account was published in Copenhagen in 1627. According to him,† "At daybreak on the 2d of September it began to thunder in the jökul; and about eight o'clock (A.M.) floods of water and ice were poured down upon the low country, and carried away upwards of 200 loads of hay,‡ which lay in the fields about Thyckvaboe. These floods continued to be poured forth like a raging sea till past one o'clock in the afternoon, when they gradually diminished, but were succeeded by terrible darkness, earthquakes, thunder, flames, and showers of sand. Nor was it in the immediate vicinity of the crater alone that the fire appeared, but down in the inhabited tract, at the distance of nearly twenty miles from the mountain, *igneous vapours were seen attaching themselves to the clothes of the inhabitants* (?). This dreadful scene continued, with little variation, till the 13th of the month. It was frequently so clear at night that the mountains, with all their clefts and divisions, were seen as distinctly at the distance of twenty miles as they were in the clearest day. Sometimes the flames were pure as the sun; sometimes they were red, and at others they discovered all the colours of the rainbow. The lightnings were visible, now in the air, and now running over the surface of the ground; and *such as witnessed them were more or less affected in such parts of their bodies as were uncovered.*[!] These flashes were accompanied by the loudest claps of thunder,* and darted backwards and forwards, now to the ground and now into the air, dividing sometimes into separate bolts, each of which appeared to be followed by a separate report; and, after shooting in different directions, they instantly collected again, when a dreadful report was heard, and the igneous appearance fell like a waterspout to the ground, and became invisible. While the showers of sand lasted, it was frequently so dark in the daytime that two individuals holding each other by the hand could not discover each other's face."† Dr Hjaltalin states that the water-floods, bearing large masses of ice, "surrounded the monastery of Thyckvaboe, with its adjacent farms, one of which was overflowed by the stream; but the people saved themselves on a high hill, where the flood could not reach them.

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* In regard to its civil government, Iceland is divided, primarily, into three provinces; secondarily, into prefectures, or sheriffdoms (*syssels*), corresponding to, but more comprehensive than, our counties; and, lastly, into parishes (*hrepps*). Over each of the first presides an *amtmann*, or lieutenant-governor; over each of the second, a *sysselmann* or sheriff; and over each of the third a *hreppstiori*, bailiff or inspector of poor.

† Henderson, vol. i. p. 311.

‡ In estimating the seriousness of such a loss, it is necessary to bear in mind that the hay harvest is, so far as the vegetable kingdom is concerned, the *only* harvest in Iceland, and that hay is almost the sole provender for horses, sheep, and cattle, during three-fourths of the year.

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* Thunder is comparatively rare in Iceland, M. Arago stating that during two years—from the autumn of 1833 to that of 1835—it was only once heard at Reykjavik; but lightning is comparatively much more common, especially in the vicinity of volcanic action, according to the testimony of Olafsen and other travellers. "The *Laptelltur*," says the Edinburgh Cabinet Library volume on Iceland, p. 64, "best known in the western parts of the island, is a very curious phenomenon, seen only in winter during a strong wind and drifting snow. At night the whole sky seems on fire with a continual lightning, which moves very slowly. This appearance frightens the natives extremely, and they often lose many of their cattle by it, as the terrified animals, running about to avoid it, fall over the rocks."

† During the eruption of Hekla in 1766, the clouds of ashes were so dense as to obscure the sunbeams and produce a darkness, through which men could find their way only by groping, in some parts of the island a hundred miles off.

The flood was followed by such heavy shots and continual thunder, that the people thought the heavens would burst to pieces, and they were surrounded with continual flashes of lightning. The pasturages were so covered with ashes and pumice, that cattle, horses, and sheep could not get any food, and were seen running about in wild confusion. During the eruption such a darkness prevailed sometimes that days were darker than nights; and it is related that showers of ashes from this eruption reached the town of Bergen in Norway, which is the greatest distance to which volcanic ashes were ever thrown from Iceland."* The account in the "Islendingur" (of 16th June, p. 45) mentions farther, that the mixed water and ice flood flowed in cascades and waves over Myrdals-sand; that the inhabitants fled to the heights for safety; that the depth of the water-flood, which surrounded the monastery of Thyckvaboe, was such that a large ocean-vessel might have sailed between the byres and the principal building, and that there was an excessive falling of sand in the district to the north-east of Kötlugjá, called the Skaptartunga. This eruption thus lasted for about twelve days, wholly destroying many farms, and partially destroying, or rendering temporarily useless, others. The damage done was greatest in the low lands to the east, north-east, and south-east of Kötlugjá.

10th, 1660—commencing on 3d November—appears scarcely to have been less formidable than the preceding eruption. Water-floods overwhelmed and destroyed the farm and church of Höfdabrekka, which latter was cast into the sea immediately adjoining, apparently by an earthquake-shock. Only such articles were saved from the buildings as could at the moment be snatched away by the clergyman, Jón Salamonsson. The quantity of sand, ashes, and sulphur, thrown out and deposited on the coast about Höfdabrekka was such, that what formerly was a depth of twenty fathoms of sea-water became at once dry land. Such is the account in the "Islendingur." Dr Hjaltalin says the clouds of pumice, ashes, and sand rendered the atmosphere in the vicinity of Kötlugjá very dark during nine days. Many farms were destroyed. Flames and ashes were ejected during the greater part of the winter. Henderson asserts that "the quantity of ice, &c., carried down by the inundation was so great, that where it was deposited it rose to the height of forty-nine fathoms above the surface of the former depositions. The church of Höfdabrekka was observed to swim among the masses of ice to a considerable distance in the sea ere it fell to pieces.* The volcano appears, with some intermission, to have erupted sand the two following years." (Pp. 312, 313.)

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* During the eruption of 1693, ashes were borne from Hekla to Faröe; in September 1845 to Orkney and the north of Scotland, seven hundred miles distant; and in 1783 from the Skaptar-jökul to Faröe.

11th, 1721, beginning at nine A.M. on the 11th May.—Dr Hjaltalin says the narrative of this eruption proceeds from certain inhabitants of the *north of Iceland*, who observed the phenomenon from a distance of about a hundred English miles! These distant witnesses state that the eruption was preceded by "heavy shots," like "shots of artillery," lasting less or more for several days, and distinctly heard by them in the north of the island. These sounds were followed by a "heavy fire" (which expression seems translatable as *vivid flames*), also visible at the great distance above named. The flames or fire were followed by clouds of ashes, so dense and so extensive as to have produced complete darkness for some hours at the remoteness of eighty or a hundred miles. The same witnesses *heard* that the eruption had destroyed several farms near the volcano itself. But the source and character of the narrative naturally lead to doubts as to its veracity. The "Islendingur" gives a much more circumstantial account; and it may here be observed that throughout the chronological history of Kötlugjá's eruptions, from the first to the last, there are many discrepancies between the accounts of the "Islendingur" and those of Dr Hjaltalin. It is impossible to decide which is the more correct, or whether either is in all respects correct. But seeing that this is a matter of doubt, I can avoid the difficulty of decision only by giving the accounts of *both*, wherever these at all differ in fulness or fact. The "Islendingur" refers to an earthquake chiefly felt in Myrdal, but extending eastward to Sidu, and westward to Fljótshlíd. About noon of the same day (11th May), the earth became fissured at various points; loud sounds were heard; and, lastly, flames with steam or smoke were seen to issue from Kötlugjá. A water-flood now descended from the volcano, bearing huge pieces of ice, resembling in bulk small islands; which icebergs sailed along as rapidly as a ship in a good breeze. These icebergs were borne by the flood from Höfdabrekka eastward to Hjörleifshöfdi and Hafrsey. One village was destroyed in the east of the Myrdals-sand district. Again, Henderson states (p. 313),* the "inundations lasted nearly three days, and carried along with them such amazing quantities of ice, stones, earth, and sand, that the sea was filled with them to the distance of three miles from the shore. The sun was darkened by the smoke and ashes which were thrown into the air; sand and pumice were blown almost over the whole island; and the ice and water desolated a considerable tract of grass land over which they flowed."

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* In explanation of this, at first sight, startling statement, it is necessary to bear in mind that the majority of the Icelandic parish churches are of very *limited dimensions*, and are constructed wholly of *wood*.

12th, 1727.—Dr Hjaltalin mentions an eruption of this date, concerning which he confesses he has been unable to find any particulars, and which he believes to have been of little intensity or importance. It is not referred to either in the "Islendingur," or by Henderson. The Edinburgh Cabinet Library volume on Iceland (p. 36)—I know not on what authority—mentions an eruption in 1753, characterised by the ejection of "every variety of volcanic matter." The eruption which follows is the most celebrated of all the outbreaks of Kötlugjá, on account alike of its grandeur, its duration, and its frightful results—an eruption which has since caused Kötlugjá to be dreaded by the Icelanders as one of their most dangerous volcanoes, if not their most dangerous one.

13th, 1755.—The eruption began about noon on the 17th October 1755, and continued, with intermissions, till 25th August 1756—its duration, therefore, being nearly a year. The "Islendingur" gives a very short reference merely; but the accounts of Dr Hjaltalin and of Henderson are comparatively full. According to Dr Hjaltalin, the eruption was preceded by a series of earthquakes,* beginning in September; they were especially severe in the north-east of Iceland, near Cape Langanes, about 150 or 180 miles distant from Kötlujgá. In this district they overthrew several farms; and in a milder degree they were felt over a considerable extent of country. The eruption itself began at 10 A.M. of 17th October, about a fortnight prior to the earthquake which destroyed Lisbon. Vivid flames shot towards the sky, accompanied by severe earthquakes, sounds like thunder, and lightnings. The volcano was enveloped in smoke or steam; showers of ashes and pumice fell constantly, while volcanic bombs were hurled high into the air. The latter must have been of great size, for they were seen bursting, and the accompanying detonating reports were heard, at a distance of upwards of a hundred miles. The days, it is said, were darker than the nights; and the flames and bombs gave so unearthly a character to the scene, that the poor inhabitants fancied the day of judgment had arrived, and that our globe was bursting into atoms. Over large tracts of country, the soil was covered with sand and ashes to a depth of two or three feet; † cattle, horses, and sheep, consequently died in great numbers. This devastation caused a famine and pestilence among the inhabitants, who perished by the hundred. The eruption was violent for fourteen days. The water-floods overflowed the district of Myrdals-sand, which is about twenty miles long and sixteen broad. Five parishes were more or less devastated, and fifty farms were destroyed. These were the more local disasters; but, in addition to this, the sand and ashes were spread over a great portion of the island, producing fatal epidemics and epizöotics, and it is said even the wild-fowl fled from many parts of the island. The earthquakes were characterised by distinct wave-like motions of the land, which fluctuated like an agitated ocean, and the same earthquake-waves were propagated from the coasts outwards to sea, to the serious damage of the shipping. Henderson says (vol. i. p. 314,), "The inhabitants of the tract about Kötlugjá were first apprised of the impending catastrophe on the forenoon of the 17th of October by a number of quick and irregular tremifactions, which were followed by three immense floods from the jökul, that completely overflowed Myrdalssand, and carried before them almost incredible quantities of ice and gravel. Masses of ice, resembling small mountains in size, pushed one another forward, and bore vast pieces of solid rock on their surface. After the rocking had continued some time, an exceedingly loud report was heard, when fire and water were observed to be emitted alternately by the volcano, which appeared to vent its rage through three apertures situated close to each other. At times the column of fire was carried to such a height that it illuminated the whole of the surrounding atmosphere, and was seen at the distance of 180 miles; at other times the air was so filled with smoke and ashes that the adjacent parishes were enveloped in total darkness. Between these alternations of light and obscurity, vast red-hot globes were thrown to a great height, and broken into a thousand pieces. The following night presented one of the most awful and sublime spectacles imaginable. An unremitting noise, like that produced by the discharge of heavy artillery, was heard from the volcano. A fiery column of variegated hues rose into the atmosphere; flames and sparks were scattered in every direction, and blazed in the most vivid manner."

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* Founding his statements on the MSS. of the surgeon Sveinn Pálsson, and on Horrebow's "Natural History of Iceland," p. 12: Lond. 1758.

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* These earthquakes were attended by, or were productive of, a considerable local elevation of land. The Edinburgh Cabinet Library volume on Iceland (p. 364, *foot-note*) says that a tract of coast, "more than two miles wide, and extending fourteen miles into the sea, was raised in three ridges from 120 to 240 feet high . . . Indeed the whole southern coast seems rising, the sea having at the Skéidará sands retired some miles Danish (each = 4½ English miles)."

† The volume on Iceland immediately above quoted (p. 359) says that the depth of volcanic sand in some of the valleys near Kötlugjá was four to six feet.

"The eruption continued with more or less violence till the 7th of November, during which period dreadful *exundations of hot water* were poured forth on the low country; and the masses of ice, clay, and solid rock, that they hurled into the sea, were so great that it was *filled to the distance of more than fifteen miles;* and in some places, where formerly it was forty fathoms deep, the tops of the newly deposited rocks were now seen towering above the water. A violent eruption happened again on the 17th of November, when the volcano remained inactive till the following year, during which it emitted fire and water five times—viz., on January 15th, June 28th and 29th, and August 12th and 25th."

"The principal damage occasioned by these eruptions consisted in the destruction of the pasture-grounds throughout the most part of the syssel (or district) . . . . . Numbers of cattle were carried away by the deluge; and the mephitic substances, with which everything was impregnated, brought on a raging mortality in different parts of the country. On the breaking forth of the water, a number of people fled for refuge to an insulated mountain called Hafrsey, where they were obliged to stay seven days without either meat or drink, and were exposed to the showers of stones, fire, and water, which fell around them. The lightning, which was very violent during the eruption, penetrated through solid rocks, and killed two people and eleven horses, three of which were in a stable. One of the persons killed was a farmer, whom it struck dead as he left the door of his house. What is remarkable, his upper clothes, which were of *wool*, wore no marks of fire, but the *linen* he had under them was burned; and when he was undressed, it was found that the *skin and flesh of his right side were consumed to the very bone.* [!] His maid-servant was struck with the lightning at the same time; and, though her clothes were instantly changed, *it continued to burn in the pores of her body, and singed the clothes she put on.* [!] She died a few days afterwards, having in the meantime suffered inexpressible pain."*

This eruption, Henderson very truly remarks, becomes the more noteworthy from "the terrible convulsions to which, at the same time, a great part of the terrestrial globe was subjected.† Not only were the British Isles rocked by repeated and violent shocks of an earthquake, houses thrown down, rocks split, and the waters of the sea and lakes* heaved up; but, in Norway, Sweden, Germany, Holland, France, and Italy, the same phenomena were experienced. Spain and Portugal, however, suffered most from the shocks. Numerous villages, convents, and churches were demolished, the largest mountains shaken from the foundations, and the low grounds inundated by the swelling and overflowing of the rivers. Lisbon, in particular, exhibited a scene the most tragical and melancholy. The most ponderous edifices were heaved up and shaken; steeples, towers, and houses thrown down; the ground and streets danced under the feet of the inhabitants; and many thousands of them were buried in the ruins. Nor was the earthquake confined to Europe. It stretched over into Barbary, and destroyed upwards of a dozen of cities on the coast of Africa. Its concussions were also felt in Persia, in the West Indies, and in America."† Sir George S. Mackenzie and Sir William J. Hooker‡ both also describe

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* Olafssen and Povelsen pp. 756–762. Quoting from the same writers (p. 763), Henderson states in another part of his vol. i. (p. 328), that "it is remarkable that the last eruption of Kötlugjá threw the *Solheima-jökul* into such violent convulsions that it rose and fell by turns, and was at last raised so high that it appeared double its former size." No such jökul as that last named is laid down in Gunnlaugsson's map. It is probably, according to Henderson's description, a southern outlier of the range which includes Eyafjalla and Myrdals jökuls, intermediate between these, more to the south, and nearer the sea.

† Authorities on earthquakes speak of the *earthquake district* of Iceland as including Great Britain and Ireland, the northern portions of France, Denmark

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and Scandinavia, and Greenland. But the earthquakes and other allied phenomena that were coincident with this eruption of Kötlugjá, seem to point at a simultaneity or contemporaneity of volcanic action in much more widely separated parts of the world. And this is borne out by coincidences connected with other volcanic eruptions in Iceland—for instance, that of Skaptar-jökul in 1783, which was nearly cotemporaneous with the celebrated Calabria earthquake of the same year. Interesting exceedingly, in relation to this subject, is the circumstance that the late Professor Edward Forbes, by his researches in Marine Zoology and in Palæontology, has shown the probability that Iceland, Faröe, the Hebrides, Ireland, Spain and Portugal were connected by "a continuous tract of land, ranging from the Azores along the line of that belt of gulf-weed, which exists between the 15th and 45th degrees of north latitude." "Memoirs of the Geolog. Survey of Great Britain," vol. i.; quoted in Professor Daubeny's standard work on volcanoes ["A Description of Active and Extinct Volcanos, of Earthquakes, and of Thermal Springs," &c., by Chas. Daubeny, M.D., F.R.S., Professor of Chemistry and Botany in the University of Oxford; 2d. ed. Lond. 1848: a work I can commend as an excellent repertory of the narratives of Icelandic, Danish, and other historians or travellers, concerning volcanic eruptions in Iceland up to the date of its publication.]

* The celebrated agitation of the waters of our own Loch Ness occurred contemporaneously with the great earthquake of Lisbon here also referred to.

† Stukesley's Philosophy of Earthquakes, 3d edit.; Lond. 1756, 8vo, pp. 9-30.

‡ Journal of a Tour in Iceland in the Summer of 1809, 2d edit. 2 vols. Lond. 1813. By Sir William Jackson Hooker, K.H., D.C.L., LL.D., &c., the present distinguished Director of the Royal Botanic Garden at Kew.

this eruption in their respective works of travel; but the incidents do not differ from those just given above. The latter writer characterises the sounds accompanying the eruption as "most frightful and horrible roarings." The illuminations at night were so vivid, "that heaven and earth seemed to be equally in a state of conflagration." On the 19th October a column of smoke issued from the volcano,—which column was black by day; but the smoke was intermixed with balls and sparks of fire, which by night lighted up the whole of the Myrdal district, while the country to the east thereof was in darkness both day and night. "Ashes fell like rain" in Faröe, 300 miles distant, and subterranean noises were heard as far as the Guldbringé and Kiosar syssels—80 to 90 miles distant.

14th, 1823.—Dr Hjaltalin's narrative seems to be founded on the testimony of the surgeon Sveinn Pálsson, whose name has been already more than once mentioned. According to him, the eruption began on the 1st, and ended on or about 25th or 26th, July, being most energetic during the first fourteen days. The phenomena were, as usual, chiefly water-floods, showers of ashes, slight earthquakes, and vivid lightnings, which latter struck several persons. Only one farm, Solheimar, was destroyed, and comparatively little damage was done elsewhere; altogether, the eruption was one of the mildest and most innocuous hitherto recorded of Kötlugjá. The "Islendingur," however, says that the eruption lasted from 22d June to 18th July; that heavy water-floods swept over Myrdals-sand; and that in September three distinguished men lost their lives in the flooded rivers [one of them—I was told by my friend Mr Sigurdur Sivertsen in Reykjavik—being the father of the present respected Dean (Pálsson) of that town]. The eruptions continued less or more throughout the summer.* This chronology brings us down to the last and recent eruption, viz. that of 1860.

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* The Edinburgh Cabinet Library volume on Iceland states, that the volcano "threw out sand and ashes, covering nearly 100 square miles of ground" (p. 37), and that this eruption was immediately preceded by one of Eyafjalla, which lasted from December 1821 to July 1823. *Vide* foot-note †, p. 17 of this paper.

15th, 1860, beginning on the 8th, and continuing till 28th or 29th May.—According to Dr Hjaltalin, the eruption was preceded for several days by earthquakes, which appear to have been comparatively local. "On the morning of the 8th May, a dark cloud was seen to rise from the mountain, which at the same moment sent forth an enormous flood of water, with very large pieces of ice, running with the water-stream into the sea; and some of the pieces of ice were so large that they were stranded at a twenty fathoms depth in the sea. . . . On the 12th of May, the flames could be seen from Reykjavik, although this town is no less than about eighty English miles distant. . . . During the evening, flashes of lightning were seen in the same direction. . . . On the 16th May the smoke was about 24,000 feet high: it was sometimes of a dark colour, but at other times it resembled steam. . . . At this time the fire was seen from several places at a distance of about eighty English miles." The "Islendingur" states that the wind being northerly during the eruption, the sand and ashes fell chiefly in Myrdals-sand, which was the direction taken also by the water-floods. Sulphur was found floating in the sea, and the fish disappeared from certain parts of the neighbouring coasts. The glare from the volcano seems to have been frequently visible from Reykjavik, the inhabitants of which betook themselves to the windmill to the south-west of the town, which commands a good view over the tops of the mountains that intervene between Reykjavik and Kötlugjá. Brief notices in the Edinburgh and Glasgow newspapers [founded on information contained in private letters from Iceland], which were otherwise, however, vague and inaccurate, contain the further information that a large quantity of cinders was mixed with the water-floods; that cinders and balls of fire, as well as smoke, were thrown up on the 11th and 12th; and that the cinders and ashes, from being carried by the wind partly to sea and partly to the neighbouring snow-fields, did comparatively little damage to the lowland farms. All testimony, indeed, goes to show that, like that of 1823, this eruption has been mild and innocuous, though the phenomena, as observed from Reykjavik, as well as its well-known devastations in former eruptions, especially those of 1625 and 1755, gave rise to extreme alarm and the most serious apprehensions among the poor inhabitants.

It needs no minute examination to discover that no two historians or descriptions give precisely the same account of the phenomena of any given eruption recorded in the foregoing chronological history. The discrepancies, indeed, as well as occasionally the very nature of the phenomena described, are sufficient to shake confidence in the entire accuracy of the record of phenomena in *any* of the fifteen eruptions above referred to. These discrepancies and sources of confusion and scepticism may be partly, at least, thus explained. It is well known all over the world that different persons, according as they are precise or imaginative, truthful or the reverse, educated or ignorant, will give different accounts of the same phenomenon. It is equally known that verbal accounts, in process of repetition, generally gain additions and modifications, which change the aspect of the original account in proportion to the number of such repetitions. And it is further recognised that litterateurs, in putting on paper narratives delivered to them by word of mouth by second parties, seldom or never give the bare facts—the naked truth it might happen to be—contained in oral descriptions, but prefer to "clothe the facts" (to use the common expression) with ornate or polished language, whereby they are intended to be made more readable and attractive, but whereby, also, a further deviation from, or obfuscation of, the truth is produced. Add to all this the statements I have already made (pages 11 and 31), and it will cease at once to be matter of surprise that a general scepticism should result, that the accuracy of particular statements in the foregoing history should be called in question, or that a scientific interpretation must be put on all the phenomena therein described. I know not, for instance, on what authority it is stated, in regard to the recent eruption of Kötlugjá, that there was on a particular day a column of smoke 24,000 feet high; and until I have competent scientific authority for the correctness of such an assertion, I for one take the liberty of doubting its accuracy. Competent scientific testimony is equally necessary before various other phenomena in the eruptions of Kötlugjá be accepted unhesitatingly as *facts.* Such, for instance, is the assertion by Henderson, under the head of the eruption of 1755, of lightning burning for some time in the pores of a woman's body! Again, among statements requiring simply a scientific interpretation, are those relating to *fire*, *flame*, *smoke*, and *steam*. Sir Charles Lyell says very truly, "When the expressions 'flame' and 'smoke' are used in describing volcanic appearances, they must generally be understood in a *figurative sense.* We are informed, indeed, by M. Abich, that he distinctly saw in the eruption of Vesuvius in 1834 the flame of burning hydrogen; but what is usually mistaken for flame consists of vapour, or scoriæ and impalpable dust, illuminated by that vivid light, which is emitted from the crater below, when the lava is said to glow with the splendour of the sun. The clouds of apparent smoke are formed either of aqueous* and other vapour, or of finely comminuted scoriæ."†

It has been observed that particular volcanoes, in different parts of the world, frequently eject particular materials; for instance, that some eject only lava, others steam and scoriæ (as those of the Andes), others water. A study of the history of the eruptions of Kötlugjá will reveal that they are characterised by the sudden and extensive *water-floods*, with their floating or intermixed ice-masses. So much so is this the case, that Henderson characterises Kötlugjá as an "*aqua*-igneous volcano" (vol. i. p. 264). Such ice-floods as those of Kötlugjá are probably formed somewhat in the following way. The volcanic heat melts that part of the icy mantle of the jökul which is in immediate contact with the soil; the adhesion of the ice to the soil is thus loosened, and a stratum of water is formed, which serves to break up and flood down the sides of the mountain the superincumbent ice. Proportioned to the suddenness and degree of the volcanic heat is the quantity of water, the relative amounts of water and ice forming the flood, and the velocity and devastating effects of such flood. It ought to be remembered that floods of this character, bearing with them huge and numerous masses of ice, arising from a jökul, whose internal heat reaches a certain degree of temperature, may suddenly devastate the lowlands at times when there is no actual volcanic eruption. It must further be borne in mind, that though one great source of devastation by and danger from such floods is their sudden appearance in unusual localities, another arises from the swelling and overflow of existing rivers by the melting of the snow and ice covering a jökul. So great is occasionally such overflow, that in 1753 the "Diupaa from the Sidu-jökul rose 200 feet above its usual level, and covered the whole district with sand, stones, and fragments of ice."* Prior to the great eruption of Skaptar-jökul in 1783, "the extreme degree, to which the earth in the vicinity of the volcano was heated, melted an immense quantity of ice, and caused a great overflow in all the rivers originating in that quarter."†

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* It is well known that the generation of immense volumes of steam and other gases is generally the first phenomenon of a volcanic eruption, and the cause, moreover, of subsequent phenomena. The sudden formation and expansion of this steam and these gases it is that causes the thunder-like explosions and noises, the ejection of lava, and of the stones, ashes, and sand, which are just lava in a different form—said form being also due to a similar cause, the expansive force of contained gases. Of all the gasiform products of volcanoes in eruption, *steam* is undoubtedly the most abundant.

† Principles of Geology, 9th edition (London, 1853), p. 378.

These floods are not quite peculiar to Kötlugjá among Icelandic volcanoes, for Oræfa is celebrated for its so-called water-eruptions; which have excited at once the admiration and terror of spectators; and Hekla, and Skaptar-jökul too, have at various times ejected, or *are said* to have ejected, water. The eruptive devastations of Oræfa "have always been confined to ashes and pumice, *with vast debacles of water*, no lava having ever issued."‡ A graphic account of one of what Henderson calls the *exundations* of Oræfa, that of 7th of August 1727, by Sjera Jón Thorlakson, a clergyman residing in the midst of the scene of devastation—Sandfell—will be found in Henderson (vol. i. p. 256). Some travellers describe the sides of Hekla as scarred with ravines, formed by torrents of water during eruptions. "On 23d May 1766, during an eruption of Hekla, a column of water was seen shooting up in the midst of the sand."§ Henderson‖ mentions an "eruption of water," apparently from the Skaptar-jökul in 1753, that is, thirty years prior to its celebrated lava eruption of 1783. And the "Edinburgh Cabinet Library" account of Iceland[*] states, that in the 13th and 14th centuries *all* the southern volcanoes were in action, when "the adjoining country was completely desolated by *floods of water mingled with ice.* During the earthquake of 1339, "a boiling fountain 140 feet in diameter" is said to have suddenly appeared.[†] Henderson's account of what seems to be the same phenomenon differs somewhat. He says, "the earth rent to a great depth; and among other collections of boiling water, which made their appearance, was a hot spring *sixty* feet in diameter."[‡] During the earthquakes of 1390–91, "the ground rent in various places, and boiling water sprang up."[§] And, lastly, during the earthquake of August 1784 [a year after the great eruption of the Skaptar-jökul]—one of the most serious earthquakes that ever occurred in Iceland—many old hot springs disappeared, while new ones as suddenly appeared, jetting their water to considerable heights. Among the Haukodal geysers —*the* Geysers par excellence of Iceland—no less than thirty-five new hot springs thus suddenly appeared.[‖] The water erupted from the craters of volcanoes, from earthquake fissures, and that of hot springs suddenly appearing during earthquakes, has probably not in all cases a common or similar origin; but it appears desirable, in order to a comprehensive view of the subject of the origin of such waters, to embrace here all three classes of phenomena.

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* Edinburgh Cabinet Library volume on Iceland, p. 363.

† Henderson, vol. i. p. 279.

‡ Edinburgh Cabinet Library volume on Iceland, p. 360.

§ Ibid. p. 30.

‖ Vol. i. p. 277.

In the eruptions of Kötlugjá, there appears to have been little or no lava thrown out, while the water-floods have never, or almost never, been absent, and have been of primary importance alike from their extent, duration, and devastating effects. These water-floods of Kötlugjá are generally so described as to lead the reader to suppose that the hot water was ejected in a stream directly from the crater, and from the crater only, just as lava, pumice, and ashes are. From the absence of competent scientific evidence, as I have already pointed out, it is impossible to say whether this occurred *in any case.* All probability is against the supposition; it is almost inconceivable that water should issue simultaneously, or alternately, with molten lava, or red-hot cinders and ashes, *from the same crater.* It appears to me much more likely that the floods in question were simply the result of the sudden melting, by subterranean heat, of the vast masses of ice and snow covering the volcano. It is easy to conceive, that torrents of hot water rushing down the sides of a volcano from some unseen and unknown point or source, prior or subsequent to, or cotemporaneously and alternately with, streams of lava or showers of ashes and pumice, should, by the terrified and ignorant inhabitants of the inundated plains below, be supposed to proceed from one and the same crater.

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[*] Edinburgh Cabinet Library volume on Iceland, p. 22. [†] Ibid. p. 163.
[‡] Henderson, vol. ii. p. 236. [§] Ibid. p. 237. [‖] Ibid. p. 238–9.

Whencesoever arising, these floods have been most formidable in their operations throughout their progress, but more especially in the plains at the foot of the volcano. Carrying with them disintegrated portions of the rocks and soil over which they have passed—from the finest mud to the most enormous rock-fragments, as well as gigantic icebergs—the mud, sand, and gravel have been deposited over great extents of country, frequently as breccias or conglomerates; sandy wastes and marshes have sprung into existence; old rivers have been filled up, and new ones, as well as lakes, formed; miles added to the coast line from encroachments on the sea; the rocky sides of valleys grooved, and scratched, and polished by the rocky flood; and the soft sides of mountains washed or rubbed away bodily; while whole hills of gravel or other materials have been elsewhere deposited. Henderson, and other travellers, who have visited the low grounds around Kötlugjá, bear ample testimony to the influence of the water-floods on the physical conformation of the said lowlands. The instances of *denudation*, on the largest scale, seem particularly noteworthy; and I am disposed to doubt whether there are to be met with in the *present* geologic era many more powerful causes of denudation than the water-floods of the Icelandic and other volcanoes. It is matter of regret that this subject, so far, at least, as it can be illustrated in Iceland, has not yet met with the attention it deserves from British geologists.

In speculating on the origin of the water-floods of Kötlugjá,—and mere speculation it must remain, so far as concerns at least bygone eruptions,—the parallel phenomena of volcanic action in other parts of the world may materially assist us. Sir Charles Lyell (in his "Principles") cites a great many instances of ejections of water, hot and cold, salt and fresh, in connection with earthquakes, volcanic eruptions, or allied phenomena, in various countries. In some of these cases, the water issued apparently directly from the crater; in others, from fissures in the soil, similar in kind, but generally infinitely inferior in size, to the gjás of Iceland; while in others, it was the result simply of the sudden melting of snow and ice by subterranean heat. The subject is one of such interest as to render it desirable that I should give a few illustrations, chiefly selected from the standard work just alluded to.

1. Destructive floods frequently arise in winter from Etna in consequence of the sudden melting of snow and ice by lava-currents. On 2d March 1755, two lava streams issued from the highest crater; "they were immediately precipitated upon an enormous mass of snow, which then covered the whole mountain, and was extremely deep near the summit. The sudden melting of this frozen mass, by a fiery torrent three miles in length, produced a frightful inundation, which devastated the sides of the mountain for eight miles in length, and afterwards covered the lower flanks of Etna, where they were less steep, together with the plains near the sea, with great deposits of sand, scoriæ, and blocks of lava."

"Many absurd stories circulated in Sicily respecting this event; such as that the water was *boiling*, and that it was vomited from the *highest crater;* that it was as *salt* as the sea, and *full of marine shells;* but these were mere *inventions*, to which Recupero, although he relates them as *tales of the mountaineers*, seems to have attached rather too much importance."*

In Sicily, during an earthquake in 1790, "several fissures sent forth sulphur, petroleum, steam and hot water, and a stream of mud, which flowed for two hours, and covered a space 60 feet long and 30 broad."† In the same island, during the earthquake of 1693, "numerous long fissures of various breadth were caused, which threw out sulphurous water; and one of them, in the plain of Catania (the delta of the Simeto), at the distance of four miles from the sea, sent forth water as *salt* as the sea."* The saline character of the water ejected, or *said to have been ejected*, from volcanoes or earthquake-fissures, has been supposed to support the idea, that the water in such cases is really *sea-water*, which has somehow been sucked into, and stored up in cavities in, the interior of the earth. Hooker † states, that after certain eruptions of Hekla, salt has been found in such quantities as to load a number of horses. But there is no evidence to prove, either that the water, which flowed in torrents down the sides of Hekla during certain eruptions was saline, or that salt was deposited by its evaporation. These, I fear, are mere *tales of the mountaineers*, like similar stories anent Etna!

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* Lyell's "Principles," p. 412.

† Ibid. p. 470.

Speaking of fissures made during the celebrated earthquake in Calabria in 1783, Lyell states, "in many instances individuals were swallowed up by one shock, and then thrown out again alive, together with large jets of water, by the shock which immediately succeeded."‡

Sir William Hamilton (in his "Campi Phlegræi," p. 27) says, "It is well attested that in the great eruption of Vesuvius A.D. 1631, several towns, among which were Portici and Torre del Greco, were destroyed by a torrent of boiling water having *burst out of the mountain with the lava*, by which thousands of lives were lost."§ Sir William's theory of the origin of this water is, that it is simply *rain-water*, which has penetrated or permeated the crust of the earth, and has made its way into huge reservoirs in its interior, in which reservoir it has been stored.

2. "Deluges are often caused in the Andes by the liquefaction of great masses of snow, and sometimes by the rending open, during earthquakes, of subterranean cavities filled with water. In these inundations, fine volcanic sand, loose stones, and other materials, which the water meets with in its descent, are swept away, and a vast quantity of mud called 'Moya'

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* Lyell's "Principles," p. 503.

† Journal of a Tour in Iceland, &c., p. 115.

‡ "Principles," p. 479.

§ Quoted by Hooker, p. 113.

is thus formed, and carried down into the lower regions. Mud derived from this source descended in 1797 from the sides of Tunguragua in Quito, and filled valleys 1000 feet wide to the depth of 600 feet, damming up rivers and causing lakes. In these currents and lakes of 'moya,' thousands of small *fish* are sometimes enveloped, which, according to Humboldt, have lived and multiplied in subterranean cavities.* So great a quantity of these fish were ejected from the volcano of Imbaburu in 1691, that fevers, which prevailed at the period, were attributed to the effluvia arising from the putrid animal matter."†

In January 1803, the vast mass of snow, which usually covers Cotopaxi, in South America, was suddenly dissolved in a night. "In Quito, on the 19th of July 1698, during an earthquake, a great part of the crater and summit of the volcano Carguairazo fell in, and a stream of water and mud issued from the broken sides of the hill."‡

During the earthquake in Peru, on 28th October 1746, a volcano broke out in Lucanas, "and such quantities of water descended from the cone that the whole country was overflowed; and, in the mountain near Pataz called Conversiones de Caxamarquilla, three other volcanoes burst out, and frightful torrents of water swept down their sides."§

"During the earthquake, which destroyed Caraccas (on 26th March 1812), an immense quantity of water was thrown out at Valecillo, near Valencia, as also at Porto Cabello, through openings in the earth; and in the Lake Maracaybo *the water sank.*"‖ The latter is the sort of coincident phenomenon, which is eagerly seized upon as an illustration of the correctness of the theory of those, who assert the connection between the water ejected in the course of volcanic eruptions or earthquakes and that of lakes or rivers. That there *may* be a connection in certain rare instances is possible; but this, not being a subject properly coming within the scope of this paper, I can only refer to it *en passant*.

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* The presence of fish has been regarded by other writers as a proof that the water ejected from volcanic craters or earthquake-fissures was originally the water of rivers, lakes, or the sea, which had been sucked into the interior of the earth by suddenly formed cracks or otherwise.

† Lyell's "Principles," pp. 348 and 469.

‡ Ibid. p. 503.

§ Ibid. p. 501.

‖ Ibid. p. 465.

In the course of the earthquakes in South Carolina and in New Madrid, Missouri, in 1811-12, "the inhabitants relate that the earth rose in great undulations; and when these reached a certain fearful height, the soil burst, and vast volumes of water, sand, and pit-coal were discharged as high as the tops of trees."*

Numerous fissures were produced in Jamaica by the earthquake of 1692. "Many people were swallowed up by these rents; some the earth caught by the middle, and squeezed to death; the heads of others only appeared above ground; and some were first engulfed, and then cast up again with great quantities of water."†

3. "The town of Chittagong, in Bengal, was violently shaken by an earthquake on the 2d of April 1762, the earth opening in many places, and throwing up water and mud of a sulphureous smell."‡

In the Runn of Cutch, India, in 1819, jets of black muddy water were ejected from fissures caused by an earthquake—the water containing "numerous pieces of wrought-iron and ship nails!"§

During the volcanic eruption in the Island of Sumbawa, in April 1815, "in the Island of Amboyna, in the same month and year, the ground opened, threw out water, and then closed again."‖

In Java, in 1822, the mountain Galongoon (or Galung Gung) suddenly became an active volcano. On the 8th October "a loud explosion was heard; the earth shook, and immense columns of *hot water and boiling mud, mixed with burning brimstone, ashes, and lapilli of the size of nuts*, were projected from the mountain like a waterspout, with such prodigious violence, that large quantities fell beyond the river Tandoi, which is forty miles distant. Every valley within the range of this eruption became filled with a burning torrent; and the rivers, swollen with hot water and mud, overflowed their banks, and carried away great numbers of the people, who were endeavouring to escape, and the bodies of cattle, wild beasts, and birds. A space of twenty-four miles between the mountain and the river Tandoi was covered to such a depth with bluish mud that people were buried in their houses, and not a trace of the numerous villages and plantations throughout that extent was visible. . . . It was remarked that the boiling mud and cinders were projected with such violence from the mountain, that, while many remote villages were utterly destroyed and buried, others much nearer the volcano were scarcely injured. . . . At the end of four days a second eruption occurred, more violent than the first, in which hot water and mud were again vomited, and great blocks of basalt were thrown to the distance of seven miles from the volcano."* †

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* Lyell's "Principles," p. 466.

† Ibid. p. 504.

‡ Ibid. p. 494.

§ Ibid. p. 464.

‖ Ibid. p. 465.

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* Lyell's "Principles," p. 430.

† Should the reader wish—further than the bibliographical references already contained in this paper enable him—to compare the accounts of the eruptions of other Icelandic volcanoes with those of Kötlugjá, he may consult the following works:—

1. "Annales Islandorum Reg.," in Langenbeck's "Scriptores rerum Danic. medii ævi," which contains an account of the earlier eruptions in Iceland.

2. "Letters on Iceland," &c., by Uno Von Troil, D.D., Chaplain to his Swedish Majesty, &c. London, 1780; the prefix whereof (pp. 18 and seq.) contains a bibliographical list of 120 works on Iceland, some of which treat wholly or partly of volcanic phenomena.

3. Bishop Finnsen's "Efterretning om Tildragelserne ved Bierget *Hekla*;" Copenhagen, 1767.

4. "Kort Beskrivelse over den nye Vulcans Ildsprudning i Vester Skaptafjeld's Syssel paa Island i aaret 1783;" being an account of the eruption of the Skaptar-jökul, by Magnus Stephenson, Etatsroed of Iceland, with engravings, 8vo; Copenhagen, 1785. Translated in Hooker's "Journal of a Tour in Iceland," vol. ii. p. 124.

5. S. M. Holme "Om Jordbranden paa Island i aaret 1783;" Copenhagen, 1784.

6. Garlieb, "Island rucksichtlich seiner Vulkane;" Freiburg, 8vo, 1819.

7. Hekla og den sidst Udbrand den 2 Septem. 1845. En Monographie: med 10 Plader Lin. 8vo, Copenhagen, 1847, by J. C. Schythe. A *brochure* on the most recent Eruption of Hekla, costing about 3s. in this country.

On the Claim of Dr Wells to be regarded as the Author of the "Theory of Dew." By CHARLES TOMLINSON, Lecturer on Science, King's College School, London.

Probably there never was a scientific treatise at once so famous and so little known, as the "Essay on Dew," by William Charles Wells, M.D., published in 1814. A second edition of this treatise appeared in 1815, and a third in 1818, containing the author's autobiography written shortly before his death, which took place in September 1817. The essay excited some discussion during the author's lifetime. Some of the leading facts, together with an epitome of his "Theory of Dew," were at once adopted in books on natural philosophy, and these have been repeated, with little or no variation, by every writer on physics down to the present time.

What then has made Wells' "Essay on Dew" so famous, if at the same time it is so little known? It is but little known, because it has long been out of print, and therefore inaccessible to the readers of popular science; it is famous, because the innumerable books on natural philosophy have referred to it with applause, but chiefly because one of our most celebrated scientific authorities has pointed out this essay as a model of inductive experimental inquiry. Sir John Herschel, in his "Preliminary Discourse on the Study of Natural Philosophy," characterises Wells' essay as "one of the most beautiful specimens we can call to mind of inductive experimental inquiry lying within a moderate compass;" and he earnestly recommends it to the student of natural philosophy "as a model with which he will do well to become familiar."*

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* Dr Lardner also, in his "Treatise on Heat" (1833), gives unbounded credit to Dr Wells. He says:—"The result of his inquiries was the discovery of the cause of the phenomena of dew, and affords one of the most beautiful instances of inductive reasoning which any part of the history of physical discovery has presented" (p. 328). In another Treatise on Heat, published in 1855, Dr Lardner still gives the whole credit of the "celebrated Theory of Dew" to Dr Wells. Dr Golding Bird, in his "Elements of Natural Philosophy," 4th edit., 1854, also gives the whole credit to Wells. French and German treatises do the same. Thus Eisenlohr "Lehrbuch der Physik," 1860, in describing the capital experiment of two thermometers, one on the ground, and the other in the air, marking different temperatures, says, "Dieser Versuch rührt von Wells her," p. 367.

No wonder, then, that Wells' "Essay on Dew" is famous. But among the thousands of readers of Herschel's discourse, probably not half a dozen have ever seen, much less read, Wells' essay. The Library of the British Museum has a copy of the second edition, and a few of the public institutions of the metropolis have copies in their libraries; but it is seldom or never met with in private scientific libraries. We have often been surprised that the fame of the work should not have led to its republication.

This essay, then, is cited as a model of inductive experimental inquiry; that is, the author is supposed to have taken up a subject, which was but obscurely known, or known erroneously, and guided by one or two leading ideas, to have instituted a number of experiments which, by their teaching, suggested others, and finally landed him on the domain of sound theory, as respects the subject in hand. Now, in a case of this kind, it is not necessary to the author's fame or originality that all his experiments should be new; it is not only right, but desirable, that he should take advantage of the labours of his predecessors and contemporaries, and enlist into his service every stray fact that is likely to assist his inquiry. But it is necessary, in adopting this course, that he carefully adhere to the law of *meum* and *tuum;* and that, when he comes to inform the world of his labours, he point out what others had done before he began his experimental inquiry; sum up honestly their results, with ample reference to books and memoirs, and show that such and such was the state of the question as he found it, and such the condition of the inquiry when he ceased to pursue it.

But surely Dr Wells adopted this latter course in his famous essay, if indeed there were anything to point out on the subject of dew except what he himself discovered. The numerous works on natural philosophy do indeed favour the common notion that Dr Wells is the author of the modern theory of dew, and that he arrived at it by the induction of a series of beautiful experiments of his own contrivance. Writers on popular science are accustomed to distil their books from those of their predecessors, and to neglect that important part of the distiller's art, namely, *rectification.* Now this process of rectification can only be conducted by referring to original memoirs, and these it is not easy to find, without considerable practical knowledge and acquaintance with the literature of the subject. Those writers who so constantly refer to Dr Wells' essay with applause, are little aware that most of its results had been published long before the author commenced his labours, and that the theory for which he has obtained so much credit, was also similarly indicated, in brief but unmistakable terms.

Now, if this statement be true, the very first place in which we ought to look for a confirmation of its truth is in the essay itself. But there we find only a loose and general reference to authorities, and a very scanty acknowledgment of other men's labours. Indeed, the style of the essay is that of a man who is announcing original discoveries. We will give one specimen, and would ask the reader whether such language as this does not entirely preclude the notion that such observations had ever been made before? He says:—"I have frequently seen during nights that were generally clear, a thermometer lying on the grass plot rise several degrees, upon the zenith being occupied only a few minutes by a cloud. On the other hand, I observed a very great degree of cold to occur on the ground, in addition to that of the atmosphere, during short intervals of clearness of sky, between very cloudy states of it."

Circumstances have led us to inquire into the history of the theory of dew, and the result of our investigation may perhaps employ half an hour of the reader's time not unprofitably. Our purpose must not however be misunderstood. We are not anxious to detract from the real merit of Dr Wells, nor to cast the smallest pebble against his admirers. The Essay on Dew is an elegant production, and proves its author to have been an earnest inquirer into nature—a good observer; and, if not quite so original as is generally supposed, we believe him to have been conscientious, and that while treading in other mens' footsteps he sometimes fancied himself to be cultivating his own clearing.

Now, in order to investigate Wells' claim to be regarded as an original discoverer we will credit him with the following six items, and then proceed to strike a balance by a careful inquiry into the debits. The chief points, then, which Dr Wells is said to have established, may be thus stated:—

1. That on clear and serene nights the surface of the earth is colder than the air some feet above it.
2. That on such nights dew, or hoar-frost, is formed.
3. That in cloudy weather the temperature of the ground approaches, and is often identical with, that of the air; and under such circumstances little or no dew is formed.
4. That screens, even of the lightest material, interposed between the ground and the clear sky, and in general whatever interrupts the view of the sky, prevents that portion of the ground thus protected from cooling below the temperature of the air.
5. That different bodies exposed to the clear sky become colder than the air—the times and amounts of cooling being in general different in different bodies.
6. That all these varied phenomena are to be accounted for on the principles of *radiation* and *condensation*, by the first of which the surface of the earth after sunset, provided the sky be clear, cools down below the temperature of the air; and by the second of which the vapour suspended in the air is reduced to the liquid state by contact with a body cooler than itself. But should the sky be clouded, or the ground be protected by means of screens, the heat radiated from the earth is reflected back again, and thus maintains the surface at or about the same temperature as that of the air.

The history of dew is a good illustration of the vicious habit of transposing cause and effect. The ancients having noticed that dew was most abundant on moonlight nights, supposed the moon to be the cause of the dew, and the poet embalmed that belief in the term *Rorifera Luna*. Aristotle nevertheless more accurately described it as a species of rain, formed in the lower atmosphere in consequence of the moisture which had been evaporated by day being condensed by the cold of night into minute drops. Bacon (Natural History, p. 866) noticed that starlight and bright moonlight nights are colder than cloudy nights. Muschenbröck regarded dew as a real perspiration of plants. Du Fay considered it to be an electrical phenomenon, since metals contract it but feebly, and metals being good conductors convey away the efficient cause of the phenomenon. The common notion, however, was that dew rose out of the ground. These are mere fancies, not steps in our history, and it is surprising that they should have been conceived, seeing that a certain amount of correct observation had already been recorded. The first point to be decided was, whether dew rose from the ground, or whether the moisture that supplied it already existed in the air. The Florentine Academicians decided that the moisture was in the air, and they even attempted to estimate its amount by cooling the outer surface of a conical glass by filling it with ice, and then noting the moisture which condensed on the surface, and endeavouring to estimate its amount. This was done more accurately by the Hon. Robert Boyle, who, in his "Experimental History of Cold" (published in 1665), determined experimentally that the beautiful exhibition of frost on the window pane is "generated of the aqueous corpuscles that, swimming up and down in the air within the room, are by the various motion that belongs to the fluid bodies as such, brought to pass along the window, and then by the vehement cold of the neighbouring external air, communicated through the glass, condensed into dew and frozen into ice." To prove this, Boyle sealed up in a phial a freezing mixture of snow and salt. "After a while, the salt beginning to melt the snow, the dew on the outside began to congeal, and being rubbed off, the hoar-frost would quickly begin to come again. This phial, for further trial, being put into a pair of scales with a counterpoise, after a while, as the vapours that wandered through the air in the warm room happened to be detained more and more upon the outside of the glass and to be there frozen, the scale wherein the glass was began to be deprest, and to shrink lower and lower; after which, by adding a little to the counterpoise, we reduced them again to an equilibrium, and yet after a while the scale that held the phial subsided again more and more." In this way Boyle collected on one occasion 18 grains of water, and on another 20 grains.

In this experiment Boyle distinctly recognises the fact, that *dew* and *hoar-frost* are formed by the precipitation of the vapour of the air upon a colder body. Looking back upon this experiment, it seems so easy for Boyle to have transferred the scene of his operations to the surface of the earth sparkling with dew, or bedecked with hoar-frost, and to have reasoned thus—"After sunset, the earth must be colder than the superincumbent air, or why this dew and hoar-frost?" But before the question could assume this simple form, there was still more than a century of observation and experiment, doubt, difficulty, and discussion, but nevertheless of steady progress to be gone through.

A real advance was however made by Le Roi* of Montpellier in 1752, who (apparently without being acquainted with Boyle's experiments) was led to doubt the truth of the received notion that dew rose from the ground, by the well-known experiment of putting ice into a dry glass in summer, when dew forms on its outer surface. Such moisture, he rightly thought, must be deposited by the air; and to test his opinion he sealed a bottle of white glass containing air at the temperature of 20° Réaumur. As evening came on and the temperature of the air declined to 15°, the interior of the bottle was bedewed in the upper part; when exposed to the cold of night (which was as low as 6° R.), there was a considerable deposit of dew within the bottle. On the next day, when the bottle shared in the warmth of the returning sun, the whole of the moisture was taken up again, and the bottle became as transparent as it had been the day before.

Le Roi also reasoned correctly on this experiment, that the quantity of moisture in the air is dependent on the temperature. Hence, by lowering the temperature there must always be a certain degree of cold at which the air will deposit a portion of its moisture, and he named this temperature the *point of saturation* of the air (*degré de saturation de l'air*). "To determine this point," he says, "I take water sufficiently cooled to precipitate the moisture of the air on the exterior surface of the vessel that contains it. I pour this water into a large glass, very dry on the outside, and plunge into it the bulb of a thermometer in order to note its temperature. I allow it to become warmer by half a degree, and then pour it into another goblet. If the outer surface of the goblet is still bedewed, I allow the water to become warmer ½° at a time, until I have seized the exact point above which there will be no further precipitation. This point is the point of saturation of the air. For example, on the 5th October 1752 the temperature of the air was 13° R.; at 5½° the surface of a cooled glass became bedewed; above this point it remained dry; below it moisture from this air was precipitated upon it, and that in greater quantity as the temperature was reduced."

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* "Sur l'Elevation et la Suspension de l'Eau dans l'Air, et sur la Rosée." Mémoires de l'Academie Royale des Sciences, 1752. Paris, 1755.

Le Roi noticed that the point of saturation, or the *dew-point* as we now call it, was often, during the day, very near the temperature of the air; but at night, when the air was many degrees colder than during the day, it was natural to suppose that on some nights it would fall below its point of saturation; and that, when such was the case, all the *moisture* in excess of that proper to the air temperature would be precipitated and form dew, and thus prove that dew is really formed by condensation of the moisture of the air. Accordingly, on the 27th September 1752 at sunset, when the air was at 17° R., and the degree of saturation 13½°, a bottle of white glass was suspended in the open air; next morning, before sunrise, its exterior surface was found to be covered with dew, the reading of the thermometer being 12½°. The result of a great many experiments of this kind was, that whenever the bottle was wet with dew, the nocturnal temperature had fallen below the point of saturation, but when the surface of the glass was dry there had been no such depression. The important observation, however, was made, that although the nocturnal temperature might fall below the point of saturation, a change of wind would sometimes prevent the formation of dew.

These are all capital observations, and deserve to be rescued from a memoir which contains other less worthy details; and the more so as Le Roi's method of determining the dew-point is precisely that which many English writers on natural philosophy assign to Dalton under the date 1802.

The next step in advance was taken by M. Pictet of Geneva, the author of some beautiful experiments on radiant heat, especially those with the conjugate mirrors. He noticed that a thermometer suspended five feet from the ground marked a *lower* temperature on clear nights than one suspended at the height of 75 feet.* The experiments which led to this result originated in the method of measuring heights by the barometer; and in order to make the necessary corrections for temperature, M. Pictet wished to ascertain the law of cooling in a vertical column of air. For this purpose he erected a mast in an open plain, with simple arrangements for quickly raising and lowering thermometers. He also placed a thermometer with its bulb buried in the earth near the foot of the mast, a circumstance which caused him to miss a great discovery, for had he simply placed the thermometer on the earth, instead of burying its bulb, he would have found that on clear nights the earth's surface is colder than the air above it, whereas he supposed it to be warmer. Passing over a number of interesting results, we may state that the readings of the two thermometers, the one at 5 feet, and the other at 75 feet from the ground, were the same at about 2 or 2½ hours after sunrise, and continued so during the hottest part of the day; but some time before sunset the readings were in the opposite direction, the lower thermometer being colder than the upper one, the difference increasing rapidly till sunset, and averaging as much as 2° Réaumur, and even more, as long as twilight lasted. This difference continued up to eleven o'clock at night, and apparently until sunrise, and not till some time after sunrise did the readings agree, and then they began to cross each other as before. This result was obtained at all seasons, during moderate wind and in cloudy weather; only in the last case the effect was much less appreciable. When the sky was completely clouded, or fog prevailed, or much wind, the difference disappeared, and the readings of the two thermometers were identical.

Pictet admits that he had always supposed the cold of evening to descend from above, and he could scarcely believe his eyes when he found the thermometer at the height of 75 feet more than 2° higher than one at five feet. He then begins to reason correctly. "It is then from the ground that this coldness proceeds" ("*C'est donc du sol que provient cette fraicheur;*") and indeed the thermometer hanging at four lines from the ground generally gave a lower reading than the one at five feet. This is all correct observation; but the buried thermometer led him astray, for this, naturally enough, indicating a higher temperature than any of the other instruments, he supposed that the earth retained a considerable portion of the heat it had acquired during the day, that a layer of air cooled by evaporation from the surface, produced the cold to the height of four lines from such surface, while at higher elevations the warmer air escaped this chilling influence.

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* The experiments which led to this result were made in 1779, and an account of them was inserted by M. de Luc in the fifth volume of his "Lettres Physiques et Morales." La Haye and Paris, 1779. M. Pictet's account is published in his "Essais de Physique." Geneva, 1790.

In January 1768, Professor AlexanderWilson of Glasgow noticed that a thermometer placed on the snow marked a temperature 8° F. lower than when suspended in the air, and that the mercury always rose a small matter when a mistiness came on, and *vice versâ.** But the most important observations bearing on the theory of dew and hoar-frost were made by Professor Patrick Wilson, son of the above; and in reading the accounts of his experiments, which were begun in 1780,† we are struck with astonishment that he should have relinquished them just as they appeared to promise such good fruit. Ingenious in devising experiments, and honest in reporting their results, we cannot but regard

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* Philosophical Transactions for 1771.

† Professor Wilson's first paper is contained in the Transactions of the Royal Society of London for 1780. It is entitled "An Account of a most Extraordinary Degree of Cold at Glasgow in January last, together with some New Experiments and Observations on the Comparative Temperature of Hoar-frost and the Air near to it, made at the Macfarlane Observatory, belonging to the College. In a Letter from Patrick Wilson, M.A., to the Rev. Nevil Maskeleyne, D.D., F.R.S. and Astronomer-Royal." There is a second and shorter paper in the Philosophical Transactions for 1781, entitled "Further Experiments on Cold," &c. But the author's most elaborate paper is contained in the first volume of the Transactions of the Royal Society of Edinburgh, published in 1788. It is entitled "Experiments and Observations upon a remarkable Cold which accompanied the Separation of Hoar-frost from a Clear Air." By Patrick Wilson, Professor of Astronomy in the University of Glasgow. (Communicated by Dr Black.) Read July 5, 1784. The paper is dated Feb. 14, 1784.

Patrick Wilson as the most distinguished of Wells' predecessors.

Professor Wilson states that severe cold having set in on the 13th January, he determined to note the temperature frequently, and to keep a register, but was induced to extend his observations by meeting with "a new phenomenon, which consisted in a constant difference of temperature of the snow, which at that time covered the fields, and that of the air a few feet above,—the snow being the coldest." While taking a note of the temperature at an open window of the Observatory, at 5·30 p.m., he observed it to be at zero F. At 6 o'clock, or half an hour later, he carried the instrument into the Observatory Park, and laid it down upon the snow, when the reading was soon reduced to −13°. He supposed this to be the temperature of the air, but remembering that this was an unusual degree of cold, it occurred to him that the snow might have become cooler than the air by evaporation. To test this idea, he instituted the same evening a series of comparative experiments, and noted them down in a table; which, being the first of the kind ever published, containing the important and interesting fact, that during clear weather the ground at night is colder than the superincumbent atmosphere, cannot fail, we think, to be examined with great interest. (The contraction gr. means *graduations* or *degrees*.)

Thursday Evening, January 13.

Below 0.

8·30 o'clock, therm. on snow pointed to	gr. −12	Therm. in air to gr.	0
9	−14	... ...	− 2
10	−14	... ...	− 4
11	−17	... ...	− 6
11·30	−18	... ...	− 6
12·30 o'clock, Friday morning	−20	... ...	− 8
1	−23	... ...	− 7
1·30	−22	... ...	− 8
2	−22	... ...	− 9
2·30	−21	... ...	− 8
3	...	... ...	− 9
3·30	...	... ...	−10
4	...	... ...	−12

Below 0—*continued.*

4·30 o'clock, therm. on snow pointed to	gr. ...	Therm. in air to gr.	−12
5	...	... ...	−12
5·30	...	... ...	−12
6	...	... ...	−14
6·30	−22	... ...	−13
7	−22	... ...	−13
7·30	−22	... ...	−13
8	−19	... ...	−10

Wilson soon found that the cause of these remarkable differences could not be attributed to the evaporation of the snow, seeing that hoar-frost was being abundantly deposited at the time when the readings were lowest. On the night of the 23d January 1780, "several things were laid out at the observatory, such as sheets of brown paper, pieces of boards, plates of metal, glasses of several kinds, &c., which all began to attract hoar-frost, seemingly as soon as they had time to cool down *to* the temperature of the air. The sheets of paper being thin and easily cooled, acquired it soonest, and when beheld by candle-light were beautifully spangled over by innumerable reflections from the minute crystals of hoar-frost which had parted from the air."

If, in the above passage the writer, instead of the little word *to* which we have printed in italics, could have written the word *below*, he would have given the true theory of the formation of dew and hoar-frost. We shall see further on, that Professor Wilson more than half suspected that this cooling of bodies below the temperature of the air was necessary to the deposition of hoar-frost.

In the course of his first experiments in 1780, Wilson did not fail to remark, that the lowest temperatures were coincident with the clearest weather, or, as he expresses it, "the stars shining with a full and steady light, like that of the planets." A basket had been filled with snow from the surface, when the temperature was + 14°. At 4 o'clock, a piece of thin fir plank about a foot square was placed on this snow, and on the plank a small plate of tin, and on the tin a thermometer, which at 5 o'clock pointed to −16°, and at 6 to −18°. It now occurred to him that the snow had been cooled by a descent of cold air

" sweeping along its surface in so thin a sheet, as not to affect the air a little higher up." To test this idea, he placed a thermometer on the snow, and the reading was −22°, that of the air above being −8°; and he then proceeded to project air on the bulb of the thermometer on the snow, by means of bellows, which had been previously cooled by lying out on the snow, the operator standing to leeward of the thermometer. After continuing to blow for two minutes, he was surprised to find that the thermometer had risen no less than 10°, the reading being −12°. The experiment was varied on another night, by fanning the snow with a sheet of brown paper, fastened to the end of a long slender stick; and the result was, that the mercury rose to nearly the same degree as that suspended in the air.

These experiments sufficiently disproved the idea of the snow being cooled by a descent of cold air. The reverse process, namely, the warming of the snow by the presence of clouds, apparently suggested the following experiment, which has been often repeated by persons who seem to have had no idea that it originated in 1780 with Patrick Wilson, instead of 1812, with Wells. At 1·45, when the thermometer on the snow was at +3°, the instrument was screened by two sheets of brown paper set up on their edges, and inclined against one another like the sides of a roof, the paper having first been cooled by exposure to the snow. At 2·15 the thermometer thus sheltered pointed to +9°, showing a rise of 6°. This capital experiment was afterwards repeated, as the author remarks, "with the same event." We may here mention, that at a later period of the inquiry, Dr Black* suggested that a gauze screen should be tried instead of the brown paper. Accordingly, Wilson says, " I fastened with packthread a piece of open gauze to a hoop of 8 inches in diameter, and an inch deep, and when the thermometers were sheltered in this manner, the quicksilver commonly rose nearly 2°."

That the air above the snow should be warmer than the snow itself was a fact sufficiently startling to these earnest conscientious men, who, in a proper philosophical spirit, tried to explain it "by known principles," and yet could not avoid "a suspicion of there being something singular and undiscovered at bottom." They not unnaturally sought to ascertain whether the higher temperature of the air extended to any great distance, and they accordingly attached a thermometer to a hook at the end of a long pole, and elevated it 24 feet. On suddenly lowering the instrument, it gave a higher reading than at the station below, where the temperature on the balustrade to the windward of the building was +10°, and when several detached bodies were coated with hoar-frost. "The result of this trial," says Wilson, "appeared more remarkable than anything which had hitherto occurred." The thermometer was taken off the pole and laid on the hoar-frost, when the mercury fell not less than 6°; this was repeated several times. The instrument was also swung round in the air, so as to bring the bulb into frequent contact with fresh particles, and still there was an elevation in temperature.

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* It should be stated that Drs Irvine and Crawford, names honourably distinguished in connection with the science of heat, personally assisted Professor Wilson in this inquiry, while Dr Black gave it the sanction of his high authority by exchanging letters on the subject with him.

After this the frost broke up and the experiments were discontinued, under the mistaken idea, which was most unaccountably allowed to prevail, that the results depended upon an excess of cold. In February of the same year 1780, the frost returned, and the capital observation was made during a thick fog, that there was "no difference whatever in the temperature of the hoar-frost and the air in its neighbourhood, both being +22° at 10 a.m. At 11 the sun broke through the fog, when the temperature of the hoar-frost was observed to be as quickly affected as that of the air.

In giving an account of his labours for this winter, Professor Wilson winds up with the following remarkable words:—"In the further prosecution of this subject, and in whatever way it may be cleared up, it is probable that we shall meet with a fine instance of the congruity of Nature in all her operations, and of the stability of those general laws which have been derived from a cautious observance of the rules of experimental philosophy. It would be going too far were we to conclude from the experiments related above *that very cold air is never disposed to deposit its contents except upon bodies as cold, or colder than itself;* and yet that this is frequently the case, seems probable from a number of common appearances." He then gives such examples as the following:—After a night of frost the slates and other thinner parts about a house will be whitened with hoar-frost, when the walls and more solid parts of the building remain quite free. In like manner, the smaller branches and twigs of trees often acquire this frozen ornament when the main branches and trunk remain naked for a long time; and, in general, any thin or detached body, capable of being easily cooled, attracts hoar-frost the soonest.

In the following year, 1781, another remarkable observation was made—namely, the greatly increased temperature of the earth beneath the snow, as compared with that of the surface, and indeed with that of the air. On the 22d of January, from 1 till 3 in the morning, the thermometer in the air at the balustrade of the east wing of the Observatory marked + 4° to + 6°, and on the snow −2° to 0°. At 1·30, the thermometer in the air, 24 feet from the ground, to windward of the house, stood at + 7°. At 3, the snow in the park, nine inches below the surface, raised the thermometer to + 14°, and at the depth of six inches, to + 24°.

The following readings, taken on the 25th, show, in a most beautiful manner, the equalisation of the temperatures on the snow and in the air under the influence of increasing clouds.

	Thermometer in Air.	Thermometer on Snow.
9·45 o'clock,	+ 10	+ 3°
10·30	+ 14	+ 4
11·30	+ 17	+ 9
12·30, cloudy,	+ 22	+ 20
1 afternoon, more cloudy,	+ 25	+ 26
1·30, cloudy all over,	+ 27	+ 27

Wilson was quite aware of the importance of providing himself with good comparable instruments; for he says, "I had now ready a set of ticklish thermometers, with naked bulbs and slender ivory scales, and all very nearly agreeing as to their dimensions and distance between the fixed points." In the observations of 1783–84, he constructs his tables with especial reference to the state of the sky. These are instructive, and we quote one or two.

December 28, 1783, *at Night*.

		Air.	Snow upon the ground.
11 o'clock,	quite clear,	+5	−7
11·30	ditto,	+4	−7
11·50	overcast,	+6	+4
12·30	more overcast,	+7	+7
	Morning.		
1·30,	turning a good deal clearer,	+8	+4
1·45,	still more so,	+8	+2
2·20	clear everywhere, except an ill-defined cloud in N.E.,	+9	+4
3	ditto, except a small ill-defined cloud near zenith,	+11	+3
3·45	Very clear, except some better defined clouds near horizon, from N. to N.E.,	+12	+4

On the 30th December he remarks, that "this night the thermometers were affected still more remarkably by the vicissitudes of the atmosphere, the snow not only acquiring the same temperature as the air when the heavens were overcast, but thereupon becoming considerably warmer." The effects here referred to depend upon the influence of wind in tending to equalise the temperature, as upon radiation in lowering it.

December 30, 1783, *at Night.*

		Air.	Snow upon the ground.
7 o'clock,	clear all over,	−1	−9
9 do.	wind gentle E.,	−4	−12
10 do.	do.,	−4	−12
11·20	Wind a little before this shifted from E. and E. by N. to S., and now some clouds in S.W.,	−1	−8
11·35	Cloudy all over, wind S.S.W., blowing out the candles,	0	+4
12	Do. do.,	0	+4
12·30	No intervals of sky, but a general uniform cloudiness, which hid all the stars,	+1	+4
	Morning.		
1·30	Ditto.	+3	+9
2	Particles of snow beginning to fall,	+6	+10
2·30	Ditto,	+6	+10
3·30	More snow falling than before,	+10	+14

We have next to notice a capital series of experiments by Wilson, on the cooling of different bodies by exposure to a clear nocturnal sky. And in order to be able to ascertain whether these bodies gained or lost in weight by the exposure, he ingeniously arranged some of them on a balanced board, which he called a snow-scale, so as to be able to weigh them after the experiment, so that should they have gained in weight he would thus completely get rid of the idea of the cold being the result of evaporation, which still continued to haunt him. He first exposed some fine flinty sand, free from dust, and some snow in the snow-scale, placing one thermometer on the sand, and another on the snow, and suspending a third in the air, when he was astonished to find the excess of cold at the surface of the sand, the thermometer in air indicating +14, that on the snow +10, and that on the sand +8; and on weighing, it was found that the sand had gained $\frac{4}{10}$ths of an ounce, and the snow nearly $\frac{3}{10}$ths. The following are the tabulated results of this admirable experiment:—

January 24, 1784, *at Night.*

	Air.	Snow scale.	Sand board.	Snow upon the ground.
10·30 o'clock, .	+14	+10	+8	...
11·15 do. .	+13	+10	+8	...
11·20 do. .	+11	+8½	+6½	+5
11·40 do. .	+12	+8½	+7	...
11·55 do. .	+10	+7½	+6	+6
12·45 do. .	+12	+9	+7	...
1·30 do. .	+12	+10	+8	+6

On this occasion another experiment was tried as to the effect of a screen in preventing the cooling of a body exposed to the sky. A little after midnight, when the temperature of the air was 12° and that of the sand 7°, a thin disk of wood, which had been cooled by exposure to the air, was placed over the sand, at the height of about 1½ inch from its surface. After an interval of seventeen minutes it was removed, when the reading of the thermometer on the sand was only 2° lower than that suspended in the air. On the same occasion, the gauze screen already referred to was tried, with a similar result.

On the next night (26th January), finely powdered wood charcoal, loose shavings of brass from a turner's lathe, a friable amalgam of mercury and tin, together with sand and snow, were exposed to the sky on circular boards, two feet in diameter, mounted on a long slender frame, set at right angles to the direction of the wind. A thermometer was placed on each substance, and the reason assigned for the experiment was the very proper one, of "sand being found to favour this refrigeration." The result of the experiment was, that all the bodies exposed, except the snow, fell full 6° below the temperature of the air, which was +12°; the sand being rather the coldest, and the snow nearest the air's temperature. It was noticed that "the hoar-frost separated from the air most discernibly on the sand and charcoal dust, though the manner of attaching itself to those substances was very different. On the sand it showed itself by making the surface all over sparkle with an infinity of minute bright points; whilst on the charcoal dust it settled without such congruity, and formed into many broken filaments of a dull hue, which here and there lay clustered as if aiming at some stellated arrangement." This is very accurately described, and agrees with what we know as to the radiating powers of the bodies in question; but we cannot help thinking that the reduction in temperature is understated, the temperatures given being probably those of the hoar-frost rather than of the sand and charcoal.

In another experiment, a quantity of mercury was exposed in an oval mahogany tea-tray, and a quantity of sand in another. A thermometer was placed on each surface, and a third instrument was suspended in the air. The following are the tabulated results of the experiment:—

January 29, 1784, *at Night.*

	Air.	Tray with Quicksilver.	Tray with Sand.	Excess of Cold on Quicksilver.	Excess of Cold on Sand.
10 o'clock, .	+4	+3	−1	1	5
10·30 " . .	+3	+2	−1	1	4
11 " .	+3	+2	−1½	1	4½
11·30 " .	+3	+1½	−1	1½	4½
12 " .	+4	+2	0	2	4

A good deal of hoar-frost settled on the wood of the trays, and the cold was attributed to this, instead of the radiation producing the hoar-frost; indeed, the conclusion drawn from these experiments is, that "at any given time the more hoar-frost the air imparts to bodies over which it passes in a clear state, the greater is the cold then produced." This is simply mistaking effect for cause; and while expressing our admiration at the beauty of the experiments, the ingenuity with which they were devised, and the candour with which they were reported, we cannot help thinking that in this long inquiry the professor had rather retrograded than advanced. With these important results before him, it is wonderful to us how Wilson could persist in the idea that they depended on an "excess of cold;" that our winters are so short, and the cold not usually severe enough, for the prosecution of "so dark a subject" which he therefore hands over to the philosophers of more rigorous climes. The fact is again and again brought before him, that it is "in the clearest and stillest nights only that the cold at the surface of the boards was observed to be most remarkable," and yet he allows the great discovery to escape him, that the deposit of moisture is a consequence, not a cause, of this cold. He discovers the highly important and suggestive fact, that different bodies freely exposed to the clear sky cool at different rates, and yet this great fact bears for him no fruit. He makes the equally suggestive discovery that the refrigeration is checked by screens, even where the material is so flimsy as gauze; and this is equally barren. Truly we can only wonder that a man having done so much should have stopped short—should have ploughed the land, sown the seed, watched the growing crop, and then have left others to possess themselves of the harvest. What is so surprising is that Wilson should not have continued his experiments and observations at different seasons, and that such men as Black, Crawfurd, and Irvine, should not have seen in these results something to suggest the laying out of thermometers on other nights after the frost had disappeared.

Another remarkable fact is, that Wilson's experiments should have been unknown to subsequent inquirers in the same field, the first of whom that has now to be mentioned is

Mr James Six, F.R.S., the inventor of a well known register-thermometer, an account of which and of experiments made with it was published in the "Philosophical Transactions," and also in the "Monthly Review."

From these sources a volume was made up and published a year after his death.* The fifth chapter of this work is headed, "How far a thermometer is liable to be affected by being placed near to or at a distance from the earth; or what difference is found in the temperature of the air at different altitudes." Mr Six was a resident of Canterbury, and took advantage of the cathedral of that place for obtaining the various altitudes required for his thermometers. He says (writing in 1783), "I took corresponding observations at different stations from the surface of the earth to an elevation of more than two hundred feet above it. On making this experiment, I was at first very much surprised to find the temperature at the upper station on some nights considerably warmer than the lower one; and, on searching for the cause of this difference, I found it to proceed from *a refrigeration which takes place on the surface of the ground in the evening and night, and more particularly so when the weather is still and clear.* For, although the earth is then more liable to be heated by the sun's rays in the daytime, and, by its superior density, capable of retaining a greater degree of it than the air, yet a diminution of heat begins to take place on its surface as soon as the sun is near setting and the dew begins to appear, and increases so as considerably to cool the lower strata of the air." Here, again, there is a substitution of effect for cause; for Six evidently attributes the cooling to the dew, instead of the dew to the cooling. To see whether the phenomenon varied with the seasons, he continued his observations for about a year, and arrived at the general conclusion that the most considerable differences were to be found when the weather was serene. He also noticed that the refrigeration is much less during thick fogs and cloudy skies;

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* "The Construction and Use of a Thermometer for showing the extremes of temperature in the atmosphere during the observer's absence, together with experiments on the variations of local heat, and other meteorological observations." By James Six, Esq., F.R.S., Maidstone, 1794.

and he says, "I have never found the glass on the ground in the night warmer, or even so warm, as those above it, except when rain or snow has continued to fall during the whole night, be the general temperature of the air either warm or cold." He also noticed that the degree of cold varied with the kind of surface on which the thermometer was placed—that "on green turf where the grass is not an inch long the diminution will be less on some nights by 1°, 2°, or 3°, than where the grass is longer, and according as the cold is more or less intense." He also found the difference between the temperature on the ground and seven feet above it to be greater during the night than between seven feet and 200 feet, except in one instance, when they were equal.

Next in the order of discovery is M. Prévost, who, in a work printed at Geneva in 1792, entitled "Recherches Physico-Mécaniques sur la Chaleur," distinctly recognises the fact that when at night the sky is clear the air is generally colder near the earth than at a higher elevation; that in spring and autumn it seldom freezes under a cloudy sky, and that often on a calm night, when a cloud passes over the zenith of the observer, the thermometer instantly begins to rise. He explains this on the doctrine of radiant heat, which, he says, passes easily through the air, but that clouds are opaque for heat as well as for light—they absorb both, or only allow it to pass slowly. The heat radiating from the earth traverses the pure atmosphere with facility, but is intercepted by clouds, which form a sort of vestment for the earth; receiving the radiant heat on their lower surfaces, they become warm there, as a garment does close to the body, and restore to the earth more heat than transparent air can do. Meanwhile the surface of the cloud cools from the ease with which it emits heat into the rarefied air above. Nor need we be surprised at the quickness with which a nocturnal cloud causes its presence to be felt, seeing that the action of radiant heat, going and returning from the earth to the cloud, and from the cloud to the earth, takes place in an indivisible instant of time.

Referring to Pictet's observation made in January 1777, on the night between the 4th and 5th, that the thermometer was at 5° Fahrenheit at 10 P.M., when the sky became clouded, and at 11 P.M. it had risen to $8\frac{3}{8}^\circ$, he states that Pictet informed him that the rise in temperature was accompanied by the appearance of a low cloud of no great extent in the zenith. He also refers to a fact, well known to agriculturists, on the almost instantaneous influence of clouds on the soil in preventing the formation of dew and hoar-frost: even where the temperatures are the same it will be found under a clear sky and not under a cloudy one. All these facts, he adds, are explicable by the theory of radiant heat, and considering the clouds to act the part of screens.

A series of experiments by M. B. Prévost belong to the year 1802–3, and are contained in the "Annales de Chimie," an. XI. He begins by citing an experiment of Du Fay, in which a glass vase placed in a silver basin and exposed to the night air sometimes becomes covered with dew while the basin remains dry, and he shows by a number of experiments "that a piece of metal placed on glass usually protects also the opposite side of the glass from the deposition of dew; and in general, that whenever the metal is placed on the warmer side of the glass, the humidity is deposited more copiously either on itself or on the glass near it; that when it is on the colder side it neither receives the humidity nor permits its deposition on the glass; but that the addition of a second piece of glass over the metal destroys the effect, and a second piece of metal restores it." It was also shown that when a number of drinking-glasses of the same size were about half or two-thirds filled with water, mercury, alcohol, oil, acid, small shot, and other bodies, and exposed to the night air, no dew was deposited on their lower part, but only above the level of the substances contained in the glasses, and that at distances ranging with their nature, it being greater for mercury than water, and greater for water than oil.

The author of these experiments attempted to account for the effects produced on the principle of conduction, for which the metals are distinguished over glass and the other substances employed. Dr Thomas Young, however, in his "Lectures on Natural Philosophy," published in 1807, gave the true explanation. He notices the fact that metals have a very weak attraction for moisture, and then, after the short summary which we have quoted above, concludes in the following words:—"It appears that from its properties with respect to radiant heat, the metallic surface produces these effects by preventing the ready communication either of heat or of cold to the glass."

Dr Young also, under the same date, 1807, refers to Prévost's former experiments, and explains the fact of a thermometer rising when a cloud passes over the place of observation by its preventing the escape of heat radiating from the earth and reflecting it back to the surface.

In thus marking the chief points respecting dew, which were established by the predecessors of Dr Wells, we have seen that long before the date of his celebrated essay, five at least of the six propositions given near the commencement of this article had been established by decisive experiments recorded in "Transactions" and journals accessible to all; and this had been done many years before Dr Wells began his labours. With respect to the sixth proposition, we have seen that M. Prévost had satisfactorily accounted for the nocturnal cooling on the principle of radiation, and had justly appreciated the influence of screens and clouds in reflecting back the heat thus emitted. Indeed we have a distinct acknowledgment by Wells of the value of Prévost's theory in explaining the phenomena of dew and hoar-frost. He says that he has adopted "the hypothesis of M. Prévost of Geneva on the constant radiation of heat by bodies in contact with the atmosphere, even at a time when they are exposed to the influence of bodies warmer than themselves; as it appears to agree perfectly with all the phenomena of the communication of heat which do not depend upon conduction. I shall hereafter make frequent use of this hypothesis."

Again, speaking of other men's labours, he says, referring to hoar-frost, "it has, I believe, from the time of Aristotle, been uniformly and according to my observations, justly considered as frozen dew. I shall therefore frequently refer hereafter to the experiments of the late Mr Patrick Wilson of Glasgow respecting it, as if they had been actually made upon that fluid. Indeed several of my experiments upon dew were only imitations of some which had been previously made upon hoar-frost by that ingenious and most worthy man."

Dr Wells states that he first began to think of the phenomena of dew in the autumn of 1784, when it seemed probable that the formation of dew was accompanied by the production of cold; that in 1788 the same idea occurred to Mr Patrick Wilson, and in the same year to Mr Six.

Now this is not quite ingenuous. Wilson's first paper, as we have seen, is inserted in the "Philosophical Transactions" for 1780; his second paper in the "Transactions" for 1781, and it is his third paper, embodying the results of the former two, with new experiments and observations, under the date of 1784, which was published in the "Edinburgh Transactions" in 1788. Wells was aware of this, for he more than once refers in his essay to Wilson's papers in the "Philosophical Transactions" for 1780 and 1781; but, in his anxiety to appear as the first in the field of inquiry, he post-dates Wilson's labours by as much as eight years, and Six's five. Moreover, we have only Wells' assertion that he began to study the subject of dew in 1784, whereas we have the fruits of Wilson's labours in 1780 and the following year. We have also Wells' admission, that although the subject was almost constantly in his thoughts, he made no attempts to investigate it experimentally until the autumn of 1811, when happening to be in the country on a calm and clear night, he laid a thermometer on the grass, which was wet with dew, and suspended another in the air two feet above it. An hour after the thermometer on the grass was 8° lower than that in the air. Still, however, he regarded the cold as an effect of the dew; and it was not till some time afterwards, on continuing to ponder on the subject, that it occurred to him that Wilson, Six, and himself, had committed an error in regarding the cold as the effect of the dew. Might it not be the cause? This idea, which contains the gist of the whole matter, slowly dawned upon him: he resumed his experiments, and arrived at the true explanation of dew and some other phenomena, his observations being "mixed with facts and opinions already published by others."

Here again we think that justice has not been rendered to

Patrick Wilson, who did not altogether regard the cold as the effect of the hoar-frost, as will be seen by the passage quoted at pp. 68, 69, *ante*.

The "Essay on Dew" did not pass unchallenged during the author's lifetime. It was noticed in the "Quarterly Review" for 1814 by no less a man than Dr Thomas Young. In this review the writer enters "a protest against the total novelty of the opinions which Dr Wells' laborious series of experiments had so amply illustrated and confirmed." He further states, that while Dr Wells "affords us complete information, not only respecting the sentiments of Aristotle and Theophrastus as to the nature and causes of dew, but also of the most distinguished philosophers of modern times; some of the works, however, of the persons whom he mentions, and some of the latest, have most unaccountably escaped his attention." He then proceeds to give an account of M. Prévost's work. It is however but justice to Dr Wells to state that this particular essay, published in 1792, was unknown to him; that in 1813 Dr Marcet, a relative of M. Prévost, lent him (Dr Wells) a copy of a work by M. Prévost, published in 1809, in which the author repeats the passages reprinted by Dr Young from the earlier work.

After quoting a number of passages from Prévost's work, Dr Young refers to his own "Lectures," published in 1807, in which he states, that "when the weather has been clear, and a cloud passes over the place of observation, the thermometer frequently rises a degree or two, almost instantaneously; this has been partly explained by considering the clouds as a vesture, preventing the escape of the heat which is always radiating from the earth, and reflecting it back to the surface." He then goes on to state, that "it is true that the theory could only be completed by the application of Professor Leslie's discoveries to the circumstances of the phenomena; but it is remarkable, that this very application was made, in a case confessedly *similar*, by the author of the same work which we have last quoted." This refers to the curious experiments of M. Prévost, under the date 1802–3, and he might also have quoted Patrick Wilson on the different cooling properties of sand, charcoal, &c. Dr Wells remarked in his essay, that the appearances observed by M. Prévost "may be easily accounted for;" whereupon Dr Young retorts, "had Dr Wells been as solicitous to attend to the labours of his cotemporaries as he has been very laudably anxious to refer to those of his predecessors, he might have said, not that the experiments of M. Prévost *might* 'be easily accounted for,' from the properties which he mentions, but that they actually had been explained in a similar manner by one of his own countrymen. There are, however, some modern philosophers, who, whether from their own fault or from that of their hearers and readers, or from both, appear to be perpetually in the predicament of the celebrated prophetess of antiquity, who always told truth, but was seldom understood, and never believed; and the author of the lectures in question has not unfrequently reminded us of the fruitless vaticinations of the ill-fated Cassandra."

Dr Wells published a reply to Dr Young's strictures in the Fifth Volume of the "Annals of Philosophy" (1815); and we now proceed to make a few extracts from it. He says, "My explanation of the immediate cause of dew is grounded on the simple fact, that bodies always become colder than the neighbouring air before they are dewed, and was consequently open to the discovery of every person since the invention of thermometers. It is true, that the next step in my theory could not have been taken without the assistance of the late discoveries of others, and this has been amply acknowledged in my essay. My health at the time of its being drawn up was in such a state, that I scarcely hoped that I should ever finish my work, and my notes were so written that no person besides myself could make use of them. I composed, therefore, in haste, and had neither leisure nor strength to search public libraries for all the works which I wished to consult."*

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* We learn from the "Memoir of his Life," dictated by Dr Wells in his last illness to his friend Mr Patrick, and prefixed to a work entitled, "Two Essays," &c., published in 1818, after the death of the author, a few particulars which throw some light on the loose manner in which the Essay on Dew was written. For example, he says,—"In 1800, I was suddenly seized with a slight fit of apoplexy. From this, however, I did not recover so far as to be enabled to return to the exercise of my profession for several months; and I never afterwards regained the complete possession of my memory. I became, too, much more unfit for the pursuits of any difficult train of thought which was

Dr Wells contends that Prévost "was ignorant that bodies on the surface of the earth become much colder than the air in a clear night; this being one of the principal facts on which my theory of dew is built."

We must here remark, that although Prévost was ignorant of this important fact, it had been made sufficiently clear by Patrick Wilson and Six, with whose experiments Dr Wells was acquainted. He states, however, that when he wrote his Essay he had not read the explanation of Prévost's experiments in Dr Young's Works. He read the fifty-first lecture and the sixtieth, but not the intermediate one which contained the explanation in question. He consulted Dr Young's work in a public library, and was in haste; and as he found no reference under *dew* in the index, he searched no farther. He admits Dr Young's conjecture as to the true explanation of Prévost's window experiments to be original. "It was most happy too, since, if admitted to be just, it completely accounted for several important circumstances in M. Prévost's experiments. If, then, its learned and ingenious author had established

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the production of another person." He then goes on to say, that he was not, so far as he could ascertain, less equal to the pursuits of his own train of thought.

Referring to his inquiry into the nature of dew, which he thought would not occupy him more than a few nights, at the house of one of his friends in the country, he says,—"I commenced it in the autumn of 1812, but soon found that I had greatly miscalculated the time which it would employ me. I determined, however, to proceed, from the natural steadiness of my disposition, which would never allow me to abandon any pursuit that I had seriously undertaken. I soon found that I was altogether unequal to it; for each night's labour fatigued me so much, that I could not undertake a second for several days after. In the meantime, my ankles began to swell in the evening, which I regarded as a mark of general weakness. At length I became so infirm, about the end of 1813, that I was absolutely obliged to give up any further visits to the country.

"In the beginning of 1814 a considerable snow having fallen, I could not resist the temptation of going for several evenings to Lincoln's Inn Fields, during a very severe frost, in order to repeat and extend some of Mr Wilson's experiments on snow. I soon, however, was obliged to desist." His symptoms became so alarming, that his friend, Dr Lister, thought he could not survive more than a few months. He says, "I set about immediately composing my Essay on Dew, as my papers containing the facts on which my theory was founded would, after my death, be altogether unintelligible to any person who should look into them. I laboured in consequence for several months with the greatest eagerness and assiduity, fancying that every page I wrote was something gained from oblivion."

its truth by facts clearly seen by himself, and had afterwards pursued the subject of dew through its various ramifications by means of the clue which would have been thus obtained, he must soon have acquired a knowledge of the theory which has lately been submitted by myself to the consideration of the learned, and which he, as a member of that body, has pronounced to be just. But I must, on the other hand, be permitted to say, that, if Dr Young, forgetting that Newton became a glass-grinder in the service of science, will neglect to employ, for the increase of natural knowledge, the slow and laborious method of observation and experiment, and will frequently exhibit his speculations in a manner unsuited to the capacities of ordinary men, he ought not to think it strange that opinions advanced by him on difficult points of philosophy are not, agreeably to his own remark at the end of the criticism, received as truths beyond doubt, and are often not understood."

This is smartly said, but cannot, we think, be admitted as a sufficient answer to Dr Young's strictures.

It will naturally be asked, What then remains for Wells? If all the phenomena had been observed, and even the theory pointed out, before he began his experiments, is there any merit at all to which he can justly lay claim? We answer, that to Wells belongs the rare merit of seeing clearly where other men saw obscurely; of grasping the whole, while other men only held detached parts; of bringing the scattered and somewhat incoherent labours of other inquirers to bear upon his own experiments, which were undertaken with clearer views, and consequently a more direct purpose, than those of his predecessors; and the final result of his long and patient inquiry was the establishment of a theory of extreme beauty and simplicity, the truth of which subsequent inquiry has only tended to confirm: not that Dr Wells is to be ranked as the author of this theory, but that his Essay was, as Dr Whewell remarks,* "one of the books which drew most attention to the true doctrine, in this country at least."

Still, however, it is but an act of justice to rescue from oblivion the claims of such men as Le Roy, Pictet, Patrick

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* History of the Inductive Sciences. 3d edit. vol. ii. 1857.

Wilson, Prévost, and others. Honour to them does not diminish the merit due to Wells. It only restores to its proper course the progress of discovery, where in this, as in other branches of science, and indeed in other relations in life, men are as mutually dependent on each other for intellectual progress as they are for the supply of their ever-recurring daily wants. It is not given to one man to begin, continue, and complete the journey into the undiscovered land unaided and alone; others have preceded him in the attempt, and have left the impressions of their footsteps on the virgin soil, which, however faint and uncertain, serve nevertheless as guides to subsequent explorers.

The Theory of Terminal Fructification in the Simple Plant, of Ovules and Pollen, and of Spores. By Dr MACVICAR, Moffat.*

It is generally admitted, and indeed is obvious to every observer, that symmetry prevails to a great extent in the forms at once of the mineral, the vegetable, and the animal kingdom. Nay, it manifests itself most beautifully in the forms and orbits of the heavenly bodies, and in the phenomena of light and colours; and in a word, symmetry, makes its apparition, more or less, everywhere. But to this, it must be confessed, that there sometimes *seem* at least to be remarkable exceptions, and that, too, where one would naturally expect symmetry most to prevail. Of these one of the most striking is this, that in a simple plant, a plant with a single axis, the fructification is always terminal, so that, morphologically viewed, the fully developed plant presents its axis to us as terminated by the ugly root at one extremity, or pole, and by the beautiful inflorescence at the other—an arrangement in which all symmetry seems set at defiance. Now, why is this? Why are the flower and fruit always produced at the upper extremity of the stem, a position which (besides its disregard of symmetry) can be reached only after the plant has survived all accidents till near the close of life, and when one would suppose that its vital energy must be nearly exhausted, and the plant consequently little fitted for accomplishing this, which is nevertheless the most critical and the most important function of animated nature?

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* Read before the Botanical Society, 13th December 1860.

The only explanation which I have seen of this phenomenon, and others of a similar nature, consists in references to the condition of vegetable life at different periods of growth, these being again referred to the season of the year, or some external influence. But it is precisely the condition of vegetable life at different periods of growth which is wanted to be explained. Life, at every epoch in the development of the plant, is no doubt always co-ordinate with the work which it has to do at the time, alway adequate to accomplish the development proper to that epoch, if the environments of the plant fulfil their part. But this fact does not explain why the development of the plant is what it is, and not otherwise. The insufficiency of such a view, as also its plausibility, is, I think, well shown by the point in hand.

The terminal position of the fructification, it is said, is owing to the exhaustion of the vital energy of the plant, and the flower is, notwithstanding its beauty, the expression of exhausted energy. And in favour of this view it is, moreover, argued, with seeming cogency, that when the branch of a fruit-tree is injured, or a plant made to grow in a poor soil, it gives flower buds, whereas if it had been left free from injury, or had been grown in a rich soil, it would have given leaf buds only. And the entire explanation is closed by an appeal to what is indeed a beautiful provision in the economy of nature, viz., that when the life of the individual is in danger, an effort is made to develop the reproductive apparatus, so as to secure the life of the species, should the individual perish. Now, such an explanation seems to have everything in its favour. At first sight it seems wholly satisfactory. But it is only at first sight. Many things soon present themselves which are against it. Thus in the higher regions of the animal kingdom, those which are analogous to the region of flowering plants in the vegetable kingdom, it is found that the reproductive apparatus attaches to the period of highest physical power and earliest maturity of form. In all the more perfect animals, the appearance of the reproductive organs in force rather anticipates the epoch of the complete development of the individual form than lags behind it till the close of life, when the vital energy is expiring, as it is said to do in reference to flowering plants. And this, too, although the organs of reproduction in animals are of very humble pretensions compared with many of their other organs, and such that it seems as if they might easily be developed at any time, however late in life, while those of plants, on the other hand—their blossoms—are so elaborate and beautiful, that they are judged by every unsophisticated observer to be the most exquisite products of vegetable nature, and certainly everything about them points to the inference that, instead of being products of an exhausted system, vegetation culminates towards their production, and bestows its highest energies in their development. It is, indeed, true that in many of the lower animals (as is familiarly exhibited by the silkworm, for instance), the reproductive power does not appear until the individual has gone through all the phases of its existence, and, as may be said, has nothing else left for it to do but to die. But it were more cogent in this case to say that the reproductive apparatus does not make its appearance till the moth appears, because it is necessary that the individual should complete its metamorphoses, and attain its full development, before it is allowed to proceed to the formation of embryos, whose part is to develop and reproduce all these metamorphoses. That the appearance of the reproductive apparatus at the time referred to cannot be ascribed to exhausted life, is clearly proved by this, that if that apparatus be not allowed to discharge its function, the life of the individual will be indefinitely postponed—postponed till the ungenial influence of external agencies force it to die. In such cases, in fact, as in all others of the same order, it were nearer the truth to say that the highest flow of life and of the organism is bestowed in developing the reproductive apparatus, and that in this apparatus vital energy is in a peculiar manner stored up, rather than to regard the development of that apparatus as an evidence and a product of exhausted energy.

As to the beautiful fact invoked above to sanction the theory of exhaustion—viz., that injury or deficient nourishment tends, in vegetable nature, to give flower-buds instead of leaf-buds—this fact, when scientifically interpreted, merely affirms that when a plant or tree is hurt or starved where it is now growing, so that it would be improvident to increase its bulk in that place, the buds, which, had the plant been in health, and in a rich soil, would have given indefinite branches, or an extension of the individual (for *a leaf-bud* is an indefinite branch-bud), are so modified as to bring the branches to a close; for such is the undoubted function of *flower-buds*. In other words, the formation of flower-buds instead of leaf-buds, in the circumstances described, only shows that the circumstances of injury and starvation are met, on the part of the vegetable economy, by an arrest of the individual thus exposed to hurt and hunger. That this arrest should secure also the development of that apparatus by which the individual plant may change its locality, or, as is commonly said, by which a future individual may be provided, and the species preserved, and the end of locomotion attained where the faculty is not bestowed by the gift of limbs, is indeed an example of that wisdom and goodness which underlie all nature, and bespeak its origin; but that wisdom is never manifested in resorting to expedients to prevent worse, as we, through our weakness, are so often obliged to do. In Nature, all is manifestation of Divine law, not only in its final, but also in its formal and its physical cause. Let it be granted that, to account for the appearance of the reproductive organs in plants when their life is threatened or nearly over, the final cause has been rightly assigned when it has been shown that, by this arrangement, the race is saved when the individual perishes, still the formal, the physical cause remains to be discovered. The doctrine of exhaustion will not do.

I now proceed to give another explanation, which to me at least seems satisfactory, because it both accounts for the terminal inflorescence, and refers this phenomenon to a general law, a law of acknowledged dominion in Nature, and in fact the foster-mother of her whole economy, and of the entire life and development of the plant. I am bent on showing that a terminal position in the fructification of a simple plant exists not in contradiction but in fulfilment of that grand morphological law, with the statement of which this communication opened, and which has been already admitted as paramount in scientific botany. I maintain, that in a plant with a simple axis, the fructification is the last and the uppermost part to appear on that axis, because the grand law of symmetry requires that it should be so.

And here it is necessary for us to make some remarks upon the properties of natural axes and their structure; for the plant is an axis with its appendages; this is its authorised definition, and that not in a merely geometrical, but in a physical, a dynamical sense. M. Auguste St Hilaire closes the first paragraph of his truly philosophical "Morphologie Végétal" with this remark, "Nous conçevons donc, dès le premier instant que le végétal soumis à une sorte de polarité, se compose de deux systèmes, l'une descendant ou souterrain, l'autre aérien ou ascendant." The definition of a plant as an axis with its appendages, is in fact a very happy one; for the term "axis" in physics is a very comprehensive one, and may be extended with much advantage to natural history. If it be asked, what is meant by axis in this large sense, it may be said that it is that eminent region in the individualised object, whether magnet, galvanic apparatus, crystal, plant, or animal, to which the other parts are naturally referred in morphological projection, and of which the most highly endowed parts are the two ends or poles, which are always either homologous or antilogous in their modes of action, and such that the axis as to form may be said to be homopolar or heteropolar. As an illustration of the former sort, the homopolar axis, a crystal of common quartz may be instanced, which, when complete, usually consists of a six-sided prism, terminated at each end by a six-sided pyramid, both pyramids being similar to each other. And to produce such crystals, whose axes have poles similar and symmetrical, is obviously the end and aim of crystalline nature. Sometimes, however, the two poles of a crystal are dissimilar to each other, as in tourmaline, &c. Now the latter have heteropolar axes, and in reference to such it is well known that their poles may often be made to display powers and activities which cannot be awoke in symmetrical crystals. And of these powers and activities it is to be remarked that they are complementary or consecutive to each other, that is, they tend in the same direction, so that what moves in at one pole moves out at the other, what enters or is attracted by one pole is sent away in the same direction or repelled by the other. And it were easy to show that this implies that heteropolar axes, if within the sphere of each other's action, cannot be permanent in nature. But they must be abundant. Thus, in consequence of the opposing directions of gravity acting from beneath, and of light and heat acting from above, while electric currents are circulating variously on the earth's crust, a homopolar, a perfectly symmetrical axis, can seldom be reached. And when, as in the case of plants, the axis is slowly developed by the accretion of fluid particles under very dissimilar conditions of existence at the two poles, perfect similarity in both poles is plainly not to be looked for. In order to this, a greater degree of homogeneity, both in the material and the environments of the axis, would be required. Even crystals, if they be *implanted*, have poles which are entirely dissimilar. It is only when they are *imbedded* or suspended in a uniform medium that an identity in their poles is attained. This identity, however, in reference to every axis, we are to regard as the limit which nature aims at; and every axis, as soon as it has made the nearest approach to similarity in its two poles which is possible in the conditions of its existence, has completed the cycle of its activities.

The mode in which this is accomplished may be conceived by taking in illustration an artificial axis with whose intimate structure we are acquainted, as for instance a sand-glass, one of its ends filled with a fluid of one density, its other with a fluid of another density—syrup and water, or carbonic acid and common air, or the like. Thus formed and filled, such a piece of apparatus represents a heteropolar axis, one-half or pole syrup, the other water, or one carbonic acid, the other common air. But leave it to itself a little, the course of nature begins to take effect upon it; particles of the matter constituting the one pole begin to leave that region, and to pass into the other pole, and so on, though with decreasing rapidity, until both poles are assimilated to each other, and the symmetry of every morsel and group of molecules throughout the whole is complete. But this limit attained, all action ceases. In this case the material of which the axis consists being fluid, we obtain no information as to the ultimate forms which both poles would otherwise assume. But we may yet once more repeat that the limit is identity in both (which, however, it might be shown, could only be realized when the axis was resolved into a sphere). With regard to the plant-axis, its construction receives but little illustration from the apparatus which has been imagined, and which is indeed very rude and vague for any purpose. Though the plant-axis is, like it, the product of fluids which are dissimilar to each other, and which flow in opposite directions, yet they are not, like it, supplied in limited quantity; they are also very dissimilar; and altogether, notwithstanding the abstract demands of the law of symmetry, we can only expect that the aerial part or pole of the plant-axis, and the terrene part or pole, shall continue to be permanently dissimilar to each other. But if so, the growth and development of the plant, both upwards and downwards, is secured, so far as the typical form of the species will permit; for it is upon the existence and maintenance of this dissimilarity that growth and development, and indeed all vital action, depend.

Let us not conclude, however, that these two parts, the terrene and the aerial, are more dissimilar than they really are. Let us see to what extent symmetry or similarity is ultimately established between them. In this way we shall reach the theory of the terminal fructification in a simple plant, and much besides. Taking the case, then, of a fully developed, simple, though typical plant, and regarding it as an organism which is appointed to grow between the earth and the air, and by material supplied by both, so as to unite, as far as possible, these dissimilar elements on their mutual confines,* let us examine it in reference to the general structure of its opposite regions, and their relations to each other in point of symmetry and similarity, the grand end of natural development, the first consideration in general morphology. And let us first direct our attention to the terrene pole or part, and that *in situ*, in the soil where it has grown, and with which, while alive itself, it is virtually, if not organically, connected. Here, then, we find *a root*, and in the earth around that root *a supply of free or discrete particles* of unknown structure, but of such a nature that they tend to enter the plant, to become part and parcel of it, and to minister to its development. The root we also find to be furnished with organs or parts, adapted to receive or suck in those discrete particles, these organs or parts at the same time taking the lead in forming the root, and therefore in fixing the plant, and in diffusing the concreted air-particles of which a plant mainly consists in the earth. These receiving, absorptive, root-terminating, and root-forming parts or organs, have been named *spongioles*. Some vegetable anatomists have treated them with high consideration as individualised organs on the roots; while others have shown a disposition to neglect them altogether. It is certain, however, that they ought not to be neglected, because it is on their presence and action that the typical plant mainly depends for its life and development. And though we may regard the *spongioles* as nothing else but the advancing parts of rootlets immediately behind their tips, yet their cellular texture, their absorptive power, and their annual character and relationship to the leaves as their product, stamp them as a special apparatus of great interest and importance. They may, at all events, be described in such terms as the following:—*Spongioles* are productions of the lower extremity of a plant, developed and nourished by the rootlets on which they appear—that is (adopting language already in use, in reference to a case which will be presently shown to be analogous), spongioles have the fibrils which they terminate as their funiculi. They are also cellular bodies, eminently absorptive of certain discrete particles, which are presented to them in the medium around, and that for the purpose of the further organisation, the future growth and development, of the plant which they terminate beneath. With regard to the form of the root, or terrene pole of the plant axis, it displays, as might be expected, a very defective symmetry, partly because it is developed in seclusion from the light, which is the grand symmetrising agent in nature, and partly because it has to form itself in a dense and irregularly resisting medium, through which it must press onwards in those directions where it meets with least resistance, whether they be symmetrically related to each other or not. Still, with regard to the symmetry of roots, it has been noted by botanists that there are two principal classes of forms, and these (if, as is necessary when their forms are to be criticised, we limit their sphere by some definite periphery—as, for instance, that of a flowerpot) are such, that the one class is accurately described as *central*, and the other as *parietal*. The root in one class, in fact, shows a tendency to maintain its unity and form as far as possible, a simple continuation of the aerial axis. It forms a tap-root, the *radix stirpata*, and is properly designated as central. In other species, on the other hand, the root soon becomes multiple, and parts from the line of the axis as soon as it is underground. It tends to spread and radiate while it seeks downwards, and thus to line the walls of the containing vessel. It forms the *radix multiceps*, or *composita*, and may be justly conceived as parietal.

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* See an article by the author, on "Vegetable Morphology," in the preceding number of this Journal.

Now, such being the termination of the terrene part of the plant-axis—such the structure and function of the nether pole, the law of symmetry calls upon us to expect that the growth of the upper part of the plant, the aerial pole, will culminate towards the development of an organism of the same order. In media so dissimilar, both in mechanical condition and chemical composition, as the earth and the air, in which the terrene and the aerial parts of the plant are respectively developed, we are not indeed to look for a mere repetition of parts similar in form, but we are called upon to look for deep-seated resemblances between them. And accordingly, on the aerial summit of the plant-axis, when it has attained its full development, what do we find but certain cellular absorptive parts or organs terminating that axis, nay, usually seeking downwards, and ultimately destined for entering the earth, thus bent on imitating those on the lower end of the plant-axis which are in the earth already? Such are the ovules! That they repeat the spongioles is clear. They are developed as the terminations of certain filamentary prolongations or fibrils of the upper extremity of the axis by which they are nourished, just as the spongioles are by the fibrils of the root which they terminate. Moreover, these nourishing fibrils (or placenta and funiculi, as they are here designated) are already classified as *central* or *parietal*, as we have found that the fibrils of roots might justly be.

But what invests the ovule-papilla, when compared with the spongiole-papilla, with chief interest, and invests our theory with importance, is the relation of both with regard to the function of absorption. That the ovule is no less truly an absorptive organ than the spongiole, is clearly proved, not only by the way in which it is speedily clothed by integuments, but by the urgency with which the pollen seeks to it and penetrates it. And this wonderful function, which is otherwise quite a mystery, our theory fully explains.

Nor is this all. Our theory also explains why the parts of the inflorescence generally, and the ovule when it has accomplished its function as an absorptive organ, and is matured as a seed, are thrown off from the parent plant. Thus, in consequence of conditions of existence which have been stated, the aerial and the terrene poles of the plant-axis must always be unlike each other. The plant-axis must be a heteropolar axis, and the action of the two poles antilogous, like those of a magnet or a crystal of tourmaline. If one pole be generally attractive, the other must be generally repulsive. Now, the terrene pole, the root, is generally attractive. The plant grows mainly by the entrance into the root and the ascent of nutritious particles found in the surrounding soil. The corresponding pole, therefore, the aerial part of the plant generally, must be repulsive of the matter thus acquired; it must tend to detach, discharge, scatter, now pollen, now petals, now foliage, now fruits. As to the last, inasmuch as they contain matured ovules, which are the representatives of the spongioles, which are terrene organs, producers of roots, so are they the last to be discharged and dissipated, which, when they are, their ovules seek the earth, and in due time produce roots themselves, and whatever else follows thereafter.

Botanists hitherto have indeed been generally contented to regard the ovules merely as buds; and, no doubt, this is true, so far as it goes. But it goes only a very short way to explain the peculiar character and functions of ovules. The fact is, that everything that is capable of development may be said to be a bud. Thus, a spongiole is a rootlet-bud. And if it be true that ovules sometimes give leaves like true buds, so also do spongioles, as shown in the experiments of Duhamel, and as happens sometimes in the course of nature. But the theory that the ovule is merely a bud is quite inadequate. Buds are not naked homogeneous absorptive organs, as ovules are. Buds are not dependent for their development on an organ absorbed into them from without, as ovules are. The connection of buds with the parts which produce them is normally permanent, that of ovules is normally temporary. Buds normally tend upwards, ovules generally downwards. As well may any little concretion of cellular tissue all over the plant be called a bud, for any such eddy of the organism may, under the requisite conditions, develop like a bud. The bud theory of ovules must therefore be admitted to be inadequate.

But is the apparition of absorptive organs on the aerial pole of the plant-axis, is the absorptive character of ovules, merely a homage to the law of symmetry? That absorptive organs on the terrene pole are of essential service, we have fully seen. And it were therefore not an unauthorised notion to affirm that the repetition of them on the other pole is merely as a product of the law of symmetry, though they were not of any special use there. The philosophic naturalist must always hold himself ready to admit the dominion of general laws, given for the good and the grandeur of the whole, however seemingly useless or even cumbrous their products may be to an individual here or there. In this case, however, we are not required to do this homage to the supremacy of law. We are invited, on the other hand, to observe one of those beautiful adaptations of structure and transformations of function appointed to meet changes in the conditions of existence, to which the exuberant variety of nature is owing.

The law of symmetry, the principle of repetition, reproduction, rejuvenescence, has been carried farther than merely to give absorptive papillæ above ground terminating placentæ and funiculi, to represent the absorptive papillæ terminating rootlets; and in being carried farther, it has rendered organs which would otherwise have been merely homologous, eminently useful also.

Thus we found that the very first condition of the existence and development of the typical plant is, that there should be supplied in the surrounding medium, the earth, and be brought into contact with the absorptive organs of the root, certain minute discrete particles, indiscernible from the dust among which they are mingled, but of such a nature, that they offer themselves to the spongioles for absorption, and being absorbed, minister to the existence of the individual plant which absorbs them; so that, if the earth were transparent as the air is, while those nutritive particles were opaque, what we should see around the root of a growing plant would be a mist, a cloud, a maze, a dance (if the medium admitted of their free motion), of little particles of a peculiar and to *the existing plant* of a most serviceable kind. Now, nothing short of a very wonderful repetition of this environment of the root, the terrene pole of the plant, this environment which imparts to the plant its standing in nature, and indeed secures to it its name of plant, the law of symmetry enables the aerial pole of the plant-axis to accomplish likewise, and thereby to provide for the existence of *a future plant.* As the plant begins its life in the dust of the earth, and has for its first and lowest function to absorb and take it in, so has it for its last and highest function to form dust, and to give it off into the air. The poles of the plant-axis being heterologous, the motions belonging to them must be consecutive: if the one take in, the other must give off. In wheat-fields at sunrise, in genial climates, the ears when in flower are wrapt in a stratum of dust to which they themselves give birth. A shower of rain falling through a pine forest in flower brings down with it a shower of dust, which afterwards lies on the ground like flowers of sulphur. Let a branch, when in flower, of the common yew or savin be shaken, it is forthwith wholly wrapt in a cloud of seeming smoke. The white of the lily is soiled by the coloured dust which the flower itself produces—so abundant is the quantity of discrete particles which the plant, as its ultimate function, produces and gives off into the ambient medium. If then the earth were as transparent as the air, and the nutritious particles around the roots of plants as opaque as pollen, what we should see around the poles of the plant-axis, both terrene and aerial, when that plant-axis is fully developed, would be a system of discrete particles offering themselves at both extremes for absorption to the absorptive organs, by which both poles of the plant-axis are terminated. The law of symmetry is not, like the botanist, content with the plant in a herbarium. It looks upon it *in situ*, and as a part of nature. It does not exhaust itself in producing ovules in the flower to represent spongioles in the root. It recognises the nutritive particles around the root as already belonging to the plant; and, as a representation and repetition of these particles, it calls for the pollen around the ovules. Or, stating its function more generally, it recognises the fact, that the matter in most immediate relation with the plant at its lowest part is in a discrete or disintegrated state, and it makes it to be the function of the highest part of the plant, in like manner, to produce discrete particles also.

Hence, also, the reason why the parts in most immediate connection with the dust-producing receptacles of the plant, the filaments terminated by the anthers, are usually filamentary in form. Both the filaments and the funiculi repeat and represent branchlets and rootlets.

But if this be the true morphology of the ovules and the pollen, if they be the product of a morphological law which has been affirmed to be co-extensive with the cosmos itself, and, under God, the cosmotectonic law, ought not similar phenomena to be found in a wider sphere than that of flowering plants merely? ought not traces at least of analogous facts to be found throughout the whole vegetable kingdom? The legitimacy of this question cannot be denied; and reason looks for an answer in the affirmative. Nor do we look in vain. We find, in fact, that the production of discrete particles, particles possessing a distinct individuality, is a function, and that the last and highest, of all the vegetable tribes which constitute the vegetable kingdom. Only, as these tribes become more simple in their organisation and are more imperfectly developed, we find, as we should expect, that the two, pollen and ovules, are represented by one sort of discrete particles, possessing in itself more or less the virtue of both. Nay, we find that such particles are produced in such quantities, even very low down in the vegetable scale, as still to render the term dust applicable to them. Thus, let a bed of mould-plant, for instance, the growth of a few days on the surface of a saucer of stale paste, be agitated, and forthwith, like the yew-tree of a thousand years, it is wrapt in a cloud of dust formed by itself, and that as its highest vital function. And, not to refer to the puff-ball and other fungi, look to the unchanging lichen, which seems the accomplice of the rock it grows upon. Its stony crust is covered with dust; and with dust also its apothecia are well stored. That similar dust is also produced abundantly in special receptacles by ferns and mosses, is too well known to require to be stated. And, in fine, all the simple growths of water also, all algæ, produce discrete particles, spores and sporangia, in abundance. Sometimes, indeed, they consist of them almost entirely.

But here it may be said, with seeming cogency, that our argument goes too far. With regard to the lichen-crust, or the moss-tuft growing on the arid rock, or the green stratum covering the wet stone, it may be said that there are no discrete nutritious particles, no mould among their roots, which the dust they produce can repeat and represent. Now, let that be granted. The difficulty only evolves the riches of the law of symmetry, and presents to us a new beauty in the economy of nature. It leads us to regard these rock-plants as rooted in the air, which, inasmuch as it consists, as well as the earth, of discrete particles, some of them nutritious, calls for discrete particles as a product of those parts of air-plants in most immediate relations with their other pole, that is, those parts by which they adhere to the rock and hold their place in nature. Accordingly, it is to be expected that, by invoking the aid of the weather, these plants too shall develop discrete particles in the rock around them, that is, they shall disintegrate the rock they grow upon and produce soil, so that, instead of contradicting the law of assimilation and symmetry, and in virtue of the inverted position, as it were, in which they grow, they should beautifully provide a bed in which plants of a higher order may grow—plants which are more efficient than they for restoring the purity of the atmosphere, while at the same time they afford more sustenance to the animals which impair that purity. Now, that lichens and mosses do accomplish a disintegrating effect upon rocks, and produce soil fit for the growth of better plants, has long been well known, and the beneficent effects of such disintegration in the economy of the world has often been pointed out. But, hitherto, in attempting to account for it, no more has been possible than to regard it as a felicitous accident in nature, or as a special benevolent institution of the Creator standing by itself, and wrought out otherwise than by law, and therefore, though expressive of the Divine goodness, yet silent as to His wisdom. For wisdom demands that everything which is to be done permanently as part in a system or economy, should be done by law, or according to law. There are some minds indeed who, as soon as they are brought to see that anything comes to pass according to law, forget, or even exclude, the idea of benevolent design, while yet the fact plainly proclaims benevolence as loudly as ever; for it is itself unchanged in its effects, and is assuredly as good as ever. When referred to law, they suppose the phenomenon to be adequately accounted for. This done, they care for nothing more, and they will often hear of nothing more. But benevolence manifesting itself in law—goodness operating by law—is plainly the highest and best, the most trustworthy and divine, of all conceivable modes of benevolence. And when we have a general law, which in its abstract statement is purely a law of form or development of stability or periodicity, without any reference to sensibility in it at all, and which is in fact perfectly inexorable, yet which is at the same time such, that wherever it rules it proclaims and establishes order and beauty, utility and enjoyment, we have in such a law all that we can conceive as most excellent and most suitable to the Divine procedure in creation. We see Supreme Intelligence, by one all-embracing glance followed by one act of volition, concerting by one thought thousands upon thousands of benevolent designs, and by one touch providing for the realisation of them all.

But to return to the pollen and the ovules of flowering plants, which led us to this pleasing digression, it follows from our theory, that when viewed simply as a production of vegetable nature, pollen, as well as ovules, belong to the same general category as the spores of the cryptogamia; and both are, as it were, at once defective and over-developed one-sided spores. And hence we are led to expect that pollen is an organism, such that, when deposited in an appropriate medium, it is capable of some sort of development. Nor does nature disappoint that expectation. It is now well known that the pollen, when placed in its appropriate bed, that is, on the stigma of the flower, and sometimes while still in its own lodge, or on the nectary or its neighbourhood, speedily enlarges and gives issue to a fungoid filament, which, when in a normal position, descends till it enters the micropyle of the ovule, and is received into its nucleus. Whether the pollen, considered as an independent organism, be capable of forming spores, which shall, in their turn, develop in the proper conditions of existence into fungi, is a question of a specific nature, which can only be determined by observation. And it certainly is worth inquiry, whether in this way many fungoid bodies, attaching to vegetable nature, and rising out of it, may not be accounted for?

It is certain, however, that while the pollen partakes so far of the nature of a spore, it differs essentially from it. It is only in reference to the external parts that a similarity can be maintained. The contents of pollen grains and spores are materially different. And in the pollen, besides those globules of oil and grains of starch which are to be found, our theory leads us to believe in at least one element in each grain, which shall resume or represent, or, according to the conception of Leibnitz, mirror, the aerial pole of the plant, the leaves and flower that produced it, with power, when supplied with adequate nutrition and the other conditions of growth, to develop into and to reproduce those leaves that flower. In the ovule, similarly, our theory leads us to believe, that as it is the last product of the root and axial part of the plant, so will there be produced in the ovule at least one individualized body mirroring the root and axis, and tending under growth to develop into root and axis. And thus our theory identifies itself so far with the current doctrine of sperm-cell and germ-cell; but with this further specification, that it regards the sperm-cell as the depository of the aerial organs of the plant, the germ-cell as that of the terrene organs and axis, yet with that interchange and harmony of features which must arise from an unification of both, and from their mutual influence upon each other, and all over; for inasmuch as those two elements from pollen and ovule are dissimilar and minute, and most probably purely molecular, they must, when they meet in the same field, unite together under the grand constitutive law of molecular action, and constitute a third body with new properties. Still, though thus united, they must, like every binary combination of dissimilar elements, form an axis whose poles are dissimilar. The axis must therefore possess active polarity, each pole aiming at the assimilation of the other to itself as best it can. And therefore, so long as such assimilation is postponed, growth and development will be insisted upon, provided nourishment be supplied. But such an axis as we have thus reached, consisting of two parts, one representing the aerial, and the other the terrene part of a plant, bent upon growth and development, and each influencing the other so as to maintain specific unity and individuality, answers to the conception of an embryo. How often our united elements, our elementary axis, may have to repeat itself, and how much to grow, before it become visible, even as germ-cell in unison with sperm-cell, not to speak of radicle, plumule, or cotyledon, I am altogether unable to conjecture. Animalcules, if they have good eyes, and their distance of distinct vision be less than ours in proportion as their size is less, may possibly see an embryo in its first elements; but to us, with all possible aid from every possible microscope, it must remain for ever "longe invisibilis."

And here we may remark, ere we close, that our morphological doctrine explains the institution of sex. Thus, supposing the higher order of organisms to throw off, as their most highly elaborated or reproductive forms, nothing but perfect cells of the same kind, either all sperm-cells or all germ-cells, or any other kind of cells, if they were all perfect and similar to each other, in that case, no development could ensue. For all normal development is evermore a movement from a lower to a higher order of form, while a perfect cell or hollow sphere, homogeneous everywhere, is already in possession of the most perfect form. It has attained the form of repose. No normal development of it is possible. A form may, indeed, be truly spherical externally, and yet be capable of development, provided it have a living interior, in which there are eyes or eddies, or contents of any kind, which are not themselves cells complete all round, and concentric with the outer cell; and such I conceive to be the structure of spores, &c. But before perfect cells, designed to represent and reproduce a compound organism, can be enabled really to do so, they must be elaborated and thrown off by that organism in sets, amounting at least to a couple, the two members of which are in some important features dissimilar to each other. Given such cells in couples, and presented to each other so that they may unite (and being dissimilar, and yet nearly related, they cannot but tend to unite), and then food being supplied, there may be development similar and equal to that of the parent or parents.

An Account of the Earthquake Shock in Cornwall on the 13th January 1860. By RICHARD EDMONDS.*

Within a period of less than seven months ending with the day above mentioned, this county has been visited with four considerable earthquake shocks, each more alarming than its predecessor, and the last as smart as any recorded here. The first two were not felt on dry land, but occasioned extraordinary agitations of the sea, which I very fully described at the last meeting of this Society, in a paper printed in the "Edinburgh New Philosophical Journal" for July last; since which I have seen Mr Mallet's fourth Report on the Facts and Theory of Earthquake Phenomena in the "British Association Report" for 1858. In his first Report he ascribed such agitations of the sea, when unaccompanied with known earthquakes, to submarine landslips. In a paper read before this Society in 1855, I showed that such an explanation was incapable of being reconciled with the facts. In his fourth Report, although he still imagines a submarine landslip to be "*a vera causa*," he asks, "is it *the* cause of any of these phenomena?" I do not see how it can be even *a* cause of any of those crestless tide-like currents which characterise these agitations, and which occupy generally from five to ten minutes in each efflux, and the same length of time in each influx.

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* Read at annual meeting of the Royal Geological Society of Cornwall on the 24th October 1860.

Since these extraordinary agitations of the sea on the 25th of June and the 4th of October 1859, on the south-western coasts of England, an earthquake shock on dry land was felt in most parts of Cornwall, including Penzance and Ludgvan, on Friday the 21st of October 1859, at 6·45 P.M., the weather throughout the island having on that day become suddenly very cold and boisterous, with furious hail-storms. A similar and an equally sudden and remarkable change of weather throughout Great Britain commenced on the 14th of December following, when a whirlwind of unusual character visited the neighbourhood of Penzance, and was succeeded on the following day by an earthquake shock in Yorkshire. An account of these phenomena I communicated to the Royal Institution of Cornwall, which was printed in the "Edinburgh New Philosophical Journal" for October last.

On the 13th of January 1860 (one lunation after the whirlwind), an earthquake shock as smart as any here on record was felt at 10·30 P.M. (local time), through nearly the whole of Cornwall from the Land's End to Callington, and from the Lizard and Mevagissy to Newquay and Wadebridge; and although its extent scarcely exceeded that of 21st of October 1859, the persons who felt it were probably twenty times as many. Like that shock, too, it was followed by another about an hour afterwards; in the former case at Truro, in the latter at Liskeard. These second shocks may serve to show why each of the two agitations of the sea above referred to was occasionally renewed before the disturbance wholly subsided.

Mr Samuel Higgs, the assistant-secretary of this Society, three days after the shock, addressed a printed circular to most of the mine agents in Cornwall, requesting to be informed (amongst other things) whether any persons under ground at the time felt "any trembling or movement of the ground, or heard any particular noise;" and if so, "the direction it came from;" and from the replies he received, I hoped he would himself have drawn up a report for this meeting. But as he preferred handing over the letters to me for that purpose, I will here quote all that is important in them. The facts unconnected with mines are derived from the newspapers, when no other source of information is mentioned.

In the Land's End district, the only places where the shock was felt, or the noise heard *underground,* are in the neighbourhood of St Ives. At St Ives Consols Mine, "one of our agents, sitting in the account-house, heard a noise like a heavy train passing, and saw a tumbler of water on the table in agitation, and two of our men underground, 130 fathoms from surface, heard a rumbling noise, and experienced a trembling sensation, and some of the shipping afloat felt as if they were going aground." At the Providence Mines, in the adjoining parish of Lelant, there was heard at the seventy-five fathom level (which is nearly 125 fathoms from the surface) "a noise as though a *kibble* (an iron bucket) had fallen into a shaft, or a *stull* (a wooden platform) had given way, but no motion was felt in the *rock.* In none of the other mines of the Land's End district was any shock felt, or sound, heard, by persons underground; whereas on the surface, in almost every locality, both the shock and the sound were observed, and in some places the shock was very alarming. At Spearne Moor Mine, in St Just, the account-house "shook so as to cause the things on the mantelpiece to tingle"—"the account-house stands on the *back* of the lode on which the mine is worked." "At Balleswidden, the sound came from the south of east, and passed away to the north of west." "At Ding Dong they heard a noise and felt a slight vibration. The weather was thick and hazy, with very little wind." At St Ives some felt their beds to rock, and the night was very dark. At Hayle, a great many persons who heard the sound concluded that a special railway train had passed at that unusual hour, and a gatekeeper jumped out of bed supposing that the gate had been dashed to pieces by some unexpected railway engine. The stoker on board H. M. ship "Bann," a flat-bottomed steamer then aground on the pier of Penzance, informed me that being below deck, he felt the ship to shake for six or eight seconds, as does a house when a heavy carriage is rolling by it over a paved road; and at the same time he heard the chain-cables by which she was secured at her bows, making a noise as if they had been dashed up and down on the rocks, on which they were partly lying. In several houses in Penzance glasses struck against each other, and in one house the floor vibrated so much, that its occupant in terror caught at some support. The chief officer of the coast-guard, in his letter printed in the following "Cornish Telegraph," says, that at Mousehole the shock appeared to travel from S. to N., and was felt throughout the town. Those in bed felt as if on board a steamer in a heavy sea. The toilet table at which his daughter was standing, appeared as if a person had taken it by one end and shaken it violently, and the various things on it positively appeared to dance. She was much alarmed, but could not move, as the floor appeared to rise and fall under her feet. The sea was greatly agitated, and directly afterwards was a heavy squall of wind and rain. The shock was particularly violent at St Michael's Mount, where plaster fell from some of the houses, and at Trengwainton, where earthenware was thrown down; at the former place the granite protrudes through the slate; at the latter, the granite again appears on the surface. In the Scilly Isles no shock was felt, nor sound heard.

Eastward of the Land's End district, and through the greatest part of Cornwall, the tremor and the noise were also very considerable. In Carrick Road, in Falmouth Harbour, "the master of a barque at anchor was so alarmed by the sudden sharp movement of his vessel, that he jumped out of his sleeping berth to ascertain the cause. The noise was heard generally through Falmouth, and was like that produced by some heavily laden vehicle, with more or less intensity of vibration of windows, china and glass. Some felt their beds rock; and one person describes the effect as that of an explosion, an undulating motion being distinctly felt on the ground-floor; and the door of an oven, which had been left open, being heard to swing to and fro, closing with a sharp noise." At Penryn and Helston the shock was also very alarming. At Ponsanooth some plaster was thrown down from a wall. At Dolcoath Mine the tremor was preceded and followed by a rumbling noise, and miners there heard the sound 260 fathoms beneath the surface. At North Wheal Crofty, "the men, at 170 fathoms from the surface, heard an unusual rumbling noise." At Redruth it was felt underground at various depths, from 10 to 190 fathoms, and appeared to proceed from S.E. by E., and to be moving towards the opposite points; many there, and at Mount Hawk, in St Agnes, "were so much alarmed, as to leave their beds in a state of bewilderment." In the United Mines, Gwennap, it was felt at the 208 fathoms level. It was also felt underground at Polberro Mine, near Par. But in most of the mines of Cornwall it was felt on the surface. At Great Busy, Chacewater, says the agent, "I heard a rumbling noise, which I thought to be the night-agent drawing some of the furniture on the wood floor of the captain's changing-room. In two seconds I felt the counting-house vibrate for ten seconds; after that, I thought one of the boilers had burst, but on looking to the engines I found all right; consequently, I supposed it to be the shock of an earthquake, or some awful crush below ground. At the western engine, one of the agents on the boiler top thought something was wrong with the boilers; my attention was called to them, and I found all was right. About thirty minutes afterwards several of the people from that district came to the engine-house, with every expectation of an explosion having occurred, each expecting something fatal, as the furniture had a most dreadful shake. It appeared to us the sound and shaking came from the west, and lasted ten or twelve seconds." At Truro, it commenced with a loud rumbling noise, like that of a heavy waggon over rough pavement; this lasted two or three seconds, and was followed by a loud dull thump or shock, like a burst of deadened thunder, causing houses to shake and glasses, &c., to dance and jingle in a startling manner; many in bed were so alarmed as to jump out. At St Dennis, "the chairs, the table (with a supper party around it), the plates and dishes on the dresser trembled, and the bells tinkled,—the watch-dog began to howl,—the sound died away gradually as it came,—the night was dark, with frequent smart showers,—wind from W.S.W., blowing at times in strong and fitful gusts. At Bodmin, a person sprang out of bed, supposing that the back wall of his house had fallen, and some miners in a neighbouring mine thought the mine was crushing together.

The agent of Trelawny Mine, in Menheniot, near Liskeard, states in reply to Mr Higgs, that nothing unusual was noticed by the miners underground, but in Liskeard and its neighbourhood the shock was very smart. His wife, as she lay in bed, heard an awful rumble, and the bedroom furniture at once partook of the oscillatory motion. One of the miners says he was literally turned from his side over on his face, as he lay in bed. Two others, returning from work, on hearing the noise, thought it proceeded from a railway train. In the village of Factory two or three pieces of earthenware fell from a dresser. A woman living close to the mine, being then in bed, thought some one under it was shaking it violently. The shock was felt also in Callington, eight miles east of Liskeard, and near the eastern boundary of Cornwall. The same letter states, that, about an hour after this shock, a second shock was felt in the writer's house by his brother, whilst sitting reading.

The barometer on the day of the shock was 30·210 at Penzance, and 30·187 at Kew, being in each case higher than for two days before, and two days after. The thermometer that night was 32·2 at Kew, and lower than for some days before, and three days after; whilst at Penzance it was 48·5, and higher than for twelve days before and a hundred days after.

I am not aware that any extraordinary agitation of the sea accompanied either of the shocks above described; and if it be asked why, when a shock is felt along an extensive line of sea-coast, it is not generally accompanied with an extraordinary agitation of the sea on such coast, it might be replied, that such agitations of the sea are usually occasioned by *vertical* submarine earth-shocks, whilst shocks felt on dry land are generally *horizontal*, or nearly so.

On a New Palæozoic Group of Echinodermata. By Professor WYVILLE THOMSON, LL.D., F.R.S.E., F.G.S., &c. (Plates III. and IV.)

Edward Forbes pointed out many years ago, the singular analogy which exists between the general laws regulating the distribution of living beings in space and in time. In the older tertiaries, most of the species are "representative" of living forms, not identical with them. In the Mezozoic formations, all the species, with the exception of some microscopic forms, are extinct, and most of the genera, and many even of the families and orders, are "representative." In the Palæozoic fossiliferous beds, those remote islands in the sea of time, examples of living genera are so exceptional, that we look with suspicion at mere external resemblances, and suspect that there may have been differences in the soft parts of the Palæozoic Trochi, Chemnitziæ, Aviculæ, &c., which may have alienated them from their modern congeners. Throughout the whole geological series the classes remain permanent; but we need only recall the Palæozoic Trilobites and Eurypteri, Cystideans, Blastoids, and Palæchini, Graptolites and rugose Corals, to be aware that the families and orders diverge far and widely from those of modern times. So widely do they differ, as to render it highly probable that a Palæozoic group, however closely it may resemble a recent one in general features, may be found, on closer examination, to present distinctive characters, sufficiently salient to necessitate its separation from its modern representative group. In palæontological research, we are little inclined to follow this somewhat trite observation to its logical conclusion. When we meet with an unknown group, even among the Silurians, it is only as a last resource that we admit the possibility of its belonging to a representative family. To attempt, in the first place, by every legitimate means, to force a new fossil group into some known recent order, may probably be the course most likely to lead to a thorough sifting and appreciation of its distinctive characters; but should this method fail, I think we may consider ourselves at liberty to cast about for a hiatus in the known zoological scale, which it may help to fill.

In the "Annals and Magazine of Natural History" for November 1857, Mr Salter briefly described a remarkable series of star-fishes, chiefly from the Lower Ludlow flags of Leintwardine. Some of these in external appearance closely approached the recent Asteriadæ; while others, in form and habit, seemed almost the counterpart of the modern brittle-stars. A subsequent examination of a more extensive and more perfect series of specimens appears to indicate the propriety of grouping all the described Silurian star-fishes, with the exception of the genus Palæodiscus (Salter), into one, or perhaps into two distinct families, allied to the Asterids, but differing in essential characters from all known recent forms.

A few months ago I detected in a large series of Leintwardine fossils, chiefly star-fishes, which I had procured through Mr Meredith, a most intelligent local collector, several forms which, at a first glance, one would have placed without hesitation among the Echinidæ; spherical bodies, with distinct ambulacra stretching from pole to pole, and a strong "lantern" of conical jaws. The whole group of Lower Ludlow Echinoderms is certainly singularly representative of modern forms. A drawer of the soft shaly flags, with Palæaster, Protaster, and Echinocystites, might almost pass for one of a Post-Pleistocene hardened clay, with impressions of Uraster, Ophiocoma, and Echinus. As in the case of the star-fishes, however, this close analogy between Echinocystites and Echinus, is more apparent than real. I shall point out in the sequel some important characters in this singular form, which have led me to the belief that it holds the same intermediate position in the zoological scale which it occupies in geological sequence, between the Sphæronites and Diploporitidæ of the Caradoc and Llandovery beds and the carboniferous Palæchinidæ. The state of preservation of the specimens hitherto procured is by no means satisfactory. They are mere impressions; usually on soft friable shale; sometimes on a harder ferruginous flag. Many obscurities may be cleared up hereafter by the discovery of more perfect specimens in a better matrix.

Echinocystites (N. G.)

The body is spherical or oval. The perisom seems to have been coriaceous. The specimens are not broken or crushed, but squeezed flat, the gentle bending of the ambulacra indicating that pressure had been applied to a flexible cyst. The surface of the spheroid is divided into five wide interambulacral areæ, stretching from the oral to the apical pole, by five narrow, double, ambulacral bands. The perisom of the interambulacral spaces is strengthened by an irregular pavement of small rounded plates, irregularly studded with tubercles, bearing short, smooth, or sometimes serrated spines. These plates are thin and scale-like—so thin, that under the pressure to which the specimens have been subjected, the round head of the spine has, in the best specimens, almost universally broken through the plate close to the tubercle, giving rise to a most deceptive appearance of pairs of pores scattered over the whole surface. It is only by a close examination that one can make out the real nature of these apertures. The spines are of two kinds, some comparatively large, nearly half an inch in length, others more numerous and thickly set, and not half the size. The ambulacra consist of two double rows of irregularly wedge-shaped pore-plates, and between these apparently a double row of minute square plates paving a narrow ambulacral avenue, the central suture being indicated by a narrow ridge. Each pore-plate is perforated by a pair of pores. I cannot make out any excavated area round the pores. The five ambulacra seem to unite at the apical pole; and though, from the friable nature of the matrix of the specimen which shows this best, there is some little confusion at the point of junction, it is evident that there is no marked apical area. The mouth, which is central with reference to the ambulacra, is a small pentagonal or stellate aperture, surrounded by dense sheaves of short spines, the tegumentary plates and the ambulacral pore-plates of the corona running close up to it. The masticatory apparatus is highly developed. As in Echinus, it consists of a five-sided pyramid, entirely within the test, and extending to about one-third of the diameter of the animal, composed of five pairs of strong, hollow, wedge-shaped jaws. The suture between each pair—that is to say, the centre of each double jaw—is interambulacral. From the state of preservation of the specimens it is impossible to determine the existence of any accessory pieces. There is no evidence of a "*torus angularis*," but the inner surfaces of the oral, or narrow ends of the jaws, are garnished with strong spines, which may represent the "*paleæ angulares*" of the Ophiurids. There is no evidence of the true, linear, "rodent" teeth of the Echinids, nor can I detect any trace of auricular processes. The anal aperture is large, indicated by a pyramidal protuberance covered by smooth irregularly wedge-shaped plates, near the margin of one of the interambulacral spaces, about a third of the diameter of the body from the mouth. This projection seems frequently to have *caught* the nearly spherical body, and to have determined its position in the mud, so that the pyramid usually projects, flattened from one side. In one specimen, which has been squeezed vertically, its position is shown most distinctly. The madreporiform tubercle is large and distinct; the surface is minutely granular; it is placed in an interambulacral space, close to the junction of the ambulacra at the apical pole.

Echinocystites pomum (n. sp.) Plate III. figs. 1–3; Plate IV. figs. 1–3.

Body spherical, or slightly flattened at the poles, from two and a-half to three inches in diameter. The "avenue" between the poriferous bands of the ambulacra distinct, and about half the width of one of the bands. Spines short and scattered.

Echinocystites uva (n. sp.) Plate IV. figs. 4, 5.

Oval, about one inch in extreme length. Ambulacral avenues very narrow, almost so indistinct as to fuse the two pore-areæ into a single band. Spines numerous, and long in proportion to the size of the body.

General Structure and Relations.—Edward Forbes, who, I believe, has taken, upon the whole, a most comprehensive and philosophical view of these organisms, saw no difficulty in the intercalation of true ambulacra between the irregularly multiplied plates of a Sphæronite, and indicated this line of passage from the large-bodied tesselated Cystideans and Diploporitidæ into the carboniferous Palæchinidæ. More recent investigations, especially the wonderful researches of Johannes Müller with Huxley's lucid comments, have greatly increased our store of facts; possibly, however, somewhat premature generalizations as to the value of distinctions between ambulacral and antambulacral poles and surfaces, the distribution of such surfaces, &c., may have obscured to some extent the deductions from our new facts. This is not the place for the discussion in full of general views. I shall rather endeavour to escape as far as possible from their influence, and to seek a clue to the true zoological position of the new forms in as close a comparison and analysis of their important parts as the state of the specimens will permit. Echinocystites resembles Echinosphærites (*Aurantium*, Wahl, &c.), and more especially an undescribed species* from the Llandovery beds of Ayrshire in its coriaceous integument, supported by small numerous plates, and in having two prominent apertures occupying nearly the same relative positions, and fulfilling, I believe, the same functions as the two principal apertures in Sphæronites. It resembles *Sphæronites pomum* (His.), some species of the genus Caryocystites, and the other Diploporitidæ, in having a system of double pores; differing from these, however, essentially, in having its double pores ranged in ambulacral bands. It resembles Agelacrinites closely, the only very essential differences consisting in the arrest, in the latter, of the ambulacra before reaching the apical pole, and the development of the deep pavement of plates beneath the ambulacral canal. In the specimen of *Agel. Buchianus* (Forbes), in Jermyn Street, there is a rudely pentagonal stamp on the apical surface, which is probably the impression of the wide base of a pyramid of jaws like that of Echinocystites, on the inside of the coriaceous integument. The two apertures are exactly in the same relative position in the two forms.

The perisom of Echinocystites seems to have been perfectly flexible, but is supported over the greater part of its surface by scale-like plates. These plates are numerous, and perfectly irregular in their arrangement in the interambulacral spaces. Towards the oral extremity they are small, and give the impression of being slightly imbricated, as in Psolus. Generally over the corona they are rounded and perfectly superficial, bearing tubercles and spines.

The Ambulacra seem to be scarcely fully calcified. The pore-plates are not closely articulated; at all events, they are not anchylosed. The series are irregular, and the pores are much larger than in the Echinidæ, reminding one—were it not for their being distinctly arranged in pairs—a pair on each plate,—of the irregular ambulacra of some Holothuriadæ. The ambulacra do not form grooves, but appear to be continuous with the superficial plates of the interambulacral spaces, and to belong to the same external calcified layer of the perisom. The ambulacral vessels were undoubtedly internal. The ambulacra are continued over the cone of jaws, close up to the mouth, a structure analogous to that in the Cidaridæ. The five ambulacra seem to meet at the apical pole. There is certainly no well-defined anal area in this position. There may be small plates with ovarian pores, but they are shown in none of my examples.

The apertures, those at all events of which we have any record in the specimens as yet procured, are two in number; one, the mouth, whose powerful masticatory armature has already been described, occupies the centre of the inferior surface; the other, at some little distance from the former, is interambulacral, and defended by an irregular cone of smooth irregular plates. This aperture I believe to be the anus, and I believe that it corresponds in function with the plated interambulacral opening in Agelacrinites, which it closely resembles, and with the valve-covered pyramid, which has been usually considered an ovarian aperture in the true Cystideans. As the nature of this orifice is a "*questio vexata*," I shall state my reasons for taking this view in full. I do not believe it to be ovarian; and my reasons apply likewise to the Cystidean pyramids. When ovarian apertures exist in the external shell of the Echinoderms, they seem usually to be merely large pores perforated each in a single plate. Their position is very variable. Frequently they perforate plates round the anus, frequently a series of special plates at the apical pole, and in some cases the ovarian pores surround the mouth. The Echinoderm ova are very small, and in no case, so far as I am aware, is there a large special common aperture for their emission. The valves of the pyramid in the Cystideans cover a large opening. In specimens in my own collection of *Echinosphærites aurantium* (Gyl.) it is open, round, and gaping. In some forms the valves seem to be habitually anchylosed, in which case there is a large, round central opening, as in some foreign species of Echinoencrinites, and in some the valves are perforated by large round pores. These pores are, I think, much more likely to be the ovarian orifices.

I can see no probability whatever in the opinion lately advocated by Mr Billings, and which has received some vague support from the writings of De Koninck and others, that the "pyramid" in the Cystideans is the mouth, and that the aperture whence the ambulacra radiate is simply an "ambulacral orifice." Such an idea appears to me to be contrary to every analogy in the class. There can be no doubt of the existence of distinct openings for the passage of the ambulacral nerves and vessels from the calyx of many of the palæozoic crinoids; but I think we must certainly assume that in this, as in all other known instances, these vessels had their origin in an annular vessel surrounding the mouth. In the whole class the œsophageal circular canal seems to be the origin and centre of the ambulacral system. It is the first part which makes its appearance in the embryo, and is so permanent and universal that one could scarcely imagine a radiating ambulacral vessel rising from any other source. The early origin of this important vascular centre, in this annular form and in this position, evidently depends upon, and is closely connected with, the origin of the nervous system in the œsophageal nerve-ring, constant in the whole invertebrate series. In Comatula and Pentacrinus, a series of expanded tegumentary plates, of the superficial system, fringe, and partly overlap, the calycine ambulacral channels. These plates seem to be very fully developed in Agelacrinites, so as to form a nearly complete archway over the ambulacral grooves. They no doubt correspond with the plates, which form a thick, close pavement in the vault of the older crinoids, and on the inner surfaces of which the grooves are channelled, in which the ambulacral vessels pass to the ambulacral orifices. In all cases among the Echinoderms, where the external layer of the integument is fully calcified, there seem to be either one or two large evident openings, surrounded by special accessory plates. When there is only one, it performs the double function of mouth and anus; and when there are two, the one from which the ambulacral grooves, where these exist, constantly radiate, is the mouth; and the other, whose position is very variable, is the anus. When there is no distinct anus, the mouth is frequently slightly eccentric—in one or two cases apparently extremely so; but this eccentricity would only cause a slight irregularity in the length and distribution of the five main vascular branches. It is impossible, however, to imagine these vessels proceeding from a ring round the "pyramid" to an aperture in the centre of the vault, and thence distributing themselves to the arms or grooves. In Echinocystites there was an intestine which may now be traced by its contents; therefore there must have been an anus. The anus must have been large and dilatable, as the intestine contains fragments of considerable size of brachiopodous shells, &c. In one of my specimens a mass of such *rejectamenta*, including small Leptenæ and Beyrichiæ, occupies the portion of the intestine immediately within the supposed anal orifice. There certainly is no room for such an aperture at the apical pole.

The *Madreporiform tubercle* is very distinct—round, with a finely granular surface. It is placed between two of the ambulacra, and close to the apical pole. From observations on the early development of Comatula, which will shortly be published elsewhere, I cannot believe that there is any connection between the Madreporiform tubercle and sand-canal and the stem. The tubercle exists apparently in all cases where the function of locomotion is actively performed through the ambulacral system, and may possibly assist in the rapid and forcible distention of the water-feet. In the Crinoids, free and fixed, in which the tubercle and canal are absent or very obscure, locomotion and prehension are performed almost entirely through the ordinary muscular system; and the "water-feet" of the brachial ambulacra seem to be principally tactile and respiratory. It is certainly remarkable that the tubercle should exist so fully developed, associated with the first appearance of locomotive ambulacra, in a form apparently only a step removed from the Sphæronites with rudimentary stems, in which it is apparently absent.

I believe that no one familiar with the forms of Palæozoic life can pass these details of structure under even a cursory consideration, without their suggesting to him the great Sphæronites of the underlying Caradoc and Bala beds, their rudimentary stems finally obliterated, and their calyces segmented by a still imperfect locomotive apparatus, which will attain its full development in the noble Palæchinites, Archæocidarites, and Melonites, of the Carboniferous shales and limestones. Edward Forbes suggested that the Sphæronites might be rudimentary forms of Echinidæ. Echinocystites seems to afford as nearly as possible one of the missing links. The Sphæronites had true crinoidal arms,—I have a new species without calycine double pores, which has them long and well developed,—and were Crinoids or a parallel group; but though undoubtedly the brachial vessels of the Crinoids are processes of the ambulacral system, I see no reason why they should not co-exist, at all events in a rudimentary form, with true calycine ambulacra. In some Cystideans they *must* co-exist with the calycine ambulacral branches which feed the double-pores of the "pectinated rhombs;" and a group of Sphæronites, the Diploporitidæ, have double pores, in depressed areæ, and most characteristic in form, scattered over the plates of the calyx. I conceive that, wherever a double pore exists, the organ protruded is intended specially for the function of respiration. It may, as in the water-feet of the Echinidæ, combine that of locomotion; but the only object of the complete vascular circle, through which ciliary motion keeps up a perpetual current, must be the aëration of the water in the vessel. An organ occupying a single pore may of course also subserve the purposes of respiration, whether it be an appendage of the ambulacral system, acting directly, or a cœcal process of the lining membrane of the test, aërating the general perivisceral fluid. A special blood-system is described in the Echinoderms; but undoubtedly the branchial appendages, and all the respiratory apparatus of the group, are connected with the ambulacral system, and I am not aware of the existence of the true respiratory double pores except in connection with branches from the water-vessels. Without a special vessel, the arrangement would be useless. It is very likely that the arms, which are tolerably well developed in the true Sphæronites with single calycine pores, may have been rudimentary in the Diploporitidæ. The arms of Crinoids are developed round the mouth, in the position of the branchial tufts of the Holothuriadæ, with which group the Crinoids, from the very earliest stages of their embryonic development, present some very remarkable analogies. The radial plates in the Crinoids are not developed, as has been imagined, from the apical pole; but when the radial canals begin to sprout round the mouth, the first radial plates are intercalated, as it were, from above, between the already formed basal and first interambulacral series, like brackets for the support of the growing arms.

Echinocystites seems to me to be one of the transition forms in the steady main line of advance of the class towards its apparently most condensed and characteristic development in Echinus. Sphæronites has lost its arms and all that remained to it of a stem, and the Diploporitidæ have got their pores ranged into line to perform the new duty of locomotion. Parting with Sphæronites, the main body finally takes leave of the Crinoids, which thence diverge to follow, to the death apparently, through the Palæozoic, Triassic, Jurassic, and all the countless *lost* ages, their own excentric ordinal idea of frittering away their whole substance in a mass of tentacles and plumes. Echinocystites is not the sole representative of its group. Agelacrinites, which seems to have made its appearance before it, and which reappears among the Blastoids in the mountain limestone, is closely allied, although our specimens of this genus are as yet scarcely sufficiently numerous and complete to furnish the details requisite to a full analysis; and the Echinoids of these early days seem to have had their Clypeastroid relatives in Mr Salter's genus Palæodiscus. It is unfortunate that our materials for the illustration of this beautiful genus are still very imperfect. All the specimens in the College Museum in Belfast, and, I believe, all those in the Ludlow Museum, are impressions, in some form or other, of the oral surface. One of my specimens (Plate IV. fig. 6) is a mould of the interior of the oral surface, and another (Plate IV. fig. 7) is a cast of the same aspect,—this latter, however, fortunately preserves some imperfect impressions of the interior of the apical perisom. A third is an impression of the outside of the oral surface; and a fourth is a mould of the last, which has carried with it and preserved the close felting of minute spines.

Palæodiscus (Salter). Plate IV. figs. 6–8.

Palæodiscus ferox (Salter), the only known species. The body is very much compressed; the oral surface is discoid, slightly arched, about an inch and a half in diameter; the mouth is central, small, furnished with a much depressed cone of five pairs of strong, double, wedge-shaped jaws, ending in double, striated chisel-like teeth; the cone is within the perisom, as the ambulacral plates pass over it, close up to the margin of the mouth. The ambulacra are five in number, slightly petaloid in form, contracted towards the mouth, and again narrowing at the margin. They are continued over the apical surface to the apical pole, but apparently not in the same petaloid form. One of my specimens shows one of the ambulacra doubling back as a linear groove, with a row of single orifices, probably for the passage of locomotive suckers on either side. The ambulacra of the oral surface are composed of a double row of linear ambulacral plates, closely ranged, and in my specimens, doubtless owing to their state of preservation, presenting no visible apertures, either through or between them. An "avenue," following the contour of the ambulacra, runs along the centre, and a central ridge indicates the suture of the ambulacral plates. A row of linear adambulacral plates fringes either side of each ambulacrum, which it nearly equals in width. These plates seem equal in number to, and to alternate with, the ambulacral pore-plates. The rest of the interambulacral space is filled up by a close-fitting pavement of smooth or finely granulated plates, irregular in number and form. These plates, as well as the plates of the ambulacra, are densely covered with a velvety pile of smooth fine spines. The apical integument seems to have been supported by irregular plates. We can say nothing, as yet, about the apertures or organs. The ambulacra have already been described. There is an obscure indication of a madreporic tubercle on one specimen near the apical junction of the ambulacra. From its general resemblance, and especially from the identity in structure and arrangement of the oral portion of the ambulacral system, I have little doubt that Palæodiscus is a flattened form of the family of which Echinocystites is the type; but a series of more complete specimens is necessary for the full elucidation of its structure and affinities. One Palæodiscus seems to have died adhering to a thin frond of seaweed, for, close to it, on the same lamina of schist, there are one or two circles of a small radiating polyzoon, very like a recent Lepralia, and round them a wide patch of the delicate chains of a Hippothoa, undistinguishable as internal casts from *H. catenularia* or *H. Patagonica.* The seaweed has not left even a stain upon the stone.

We have a fragment (Plate IV. fig. 8) of apparently another species too indistinct for full description. It is distinguished by its large, regular, lozenge-shaped inter-ambulacral plates; by the more distinctly petaloid form of the ambulacra; by the width of the inter-ambulacral avenues, and by the narrowness of the pore-bearing bands. The ambulacral double-pores are well marked.

EXPLANATION OF THE PLATES.

Plate III.

Figs. 1 and 2. *Echinocystites pomum.*—Internal impressions of the oral and apical hemispheres of the same individual. In the Queen's College Museum, Belfast.

Fig. 3. *Echinocystites pomum.*—Impression of the internal surface; 3 *a*, one of the pairs of jaws enlarged. This specimen is in the Jermyn Street Museum.

Plate IV.

Fig. 1. *Echinocystites pomum.*—In the Queen's College Museum, Belfast.

Fig. 2. Portion of the impression of the outer surface of the test, enlarged to twice the natural size, and slightly restored, from a fragment in the Ludlow Museum. For the use of this, and of several other specimens, I am indebted to the kindness of Mr R. Lightbody of Ludlow.

Fig. 3. A single plate and spine, much enlarged.

Figs. 4 and 5. *Echinocystites uva.*—In the Queen's College Museum, Belfast.

Figs. 6 and 7. *Palæodiscus ferox* (Salter).—In Queen's College Museum, Belfast.

Fig. 8. *Palæodiscus gothicus* (n. sp.)—In Queen's College Museum, Belfast.

An Account of Two Artificial Hemispheres representing graphically the Distribution of Temperature and Magnetism from the Earth's Equator to the North Pole. By JAMES D. FORBES, D.C.L., LL.D., Principal of the United Colleges of St Salvator and St Leonard's, St Andrews, late Professor of Natural Philosophy in the University of Edin.

These representations of the Climate and Magnetism of the more important and accessible of the two terrestrial hemispheres, have been drawn on a plan somewhat new by Messrs W. and A. K. Johnston of Edinburgh, under my direction and superintendence. They were intended for the Natural Philosophy class, as illustrations of my lectures. The diameter of the hemispheres is thirty inches. They are mounted on square flat boards.

The advantage of delineating the climatological and magnetic curves upon the surface of a sphere instead of on Mercator's charts (as is most usually done), is well known. Indeed, unless by employing two different projections, it is impossible to form a correct idea of the totality of these complicated curves.

Another difficulty is to represent on one surface several different systems of lines, so that they may distinctly convey to the eye the collective results for each independent element. With only two systems of lines this may be done easily enough; but when a third is introduced, there is commonly much confusion. The plan which has been successfully adopted is the following:—

On the Climatological hemisphere, the common isothermals from 5° to 5° centigrade, or from 9° to 9° of Fahrenheit (taken from M. Dove's charts), are represented by solid shades of sepia, increasing in intensity with the degree of cold. By this means the deepest shadow covers only an insignificant part of the Arctic regions. The isothermals for January and July respectively are represented by curves of bright blue and bright red, which neither interfere with one another nor with the brown shading.

In the magnetical hemisphere, the sepia tinting is very suitably appropriated to the increasing gradations of total magnetic intensity from 0·9 to 1·85 of Baron Humboldt's Unit of force, which gives, for the total intensity at Paris, the number 1·348. The two remaining elements of Dip and Declination are represented by blue and red lines respectively. As to the last of these elements, the position of the horizontal needle at any point of the earth's surface, I have preferred the system of lines drawn everywhere parallel to the needle's direction, to the more artificial system of lines passing through points of equal magnetic variation or declination, according to the method of Halley. The former mode of graphical representation has the advantage of possessing a more distinct physical significance. I have, however, added to the preceding three systems the curve of No Variation, which does not interfere with any of the others, and, being shaded with green and purple on the two sides, distinguishes the regions of easterly from those of westerly declination.

As to the sources from which the previously described lines have been taken, the isothermals have all been derived from the most recent charts of M. Dove: namely, for the lower latitudes, the chart accompanying his work "On the Distribution of Heat over the Surface of the Globe;"* and for latitudes of 50° and upwards, the chart on a polar projection included in his "Klimatologische Beiträge."† I examined Mr Blodgett's charts of the isothermal lines of North America, with a view to make use of them; but I was not satisfied that they could be considered strictly comparable with the charts of M. Dove; and in particular, it appeared to me that a reduction to the level of the sea had not been allowed for; at least sufficiently, if at all.

The isodynamic Magnetic Curves, as far as latitude 60°, are taken from General Sabine's chart of 1856, in Johnston's Physical Atlas, together with the Magnetic Survey of North America by the same author in the Philosophical Transactions for 1846. For the extreme arctic latitudes, the curves have been completed partly from M. Gauss's Theoretical Charts, partly from the empirical Polar charts of M. Hansteen* and General Sabine.† The lines of dip, as far as latitude 60°, are taken, like the last, from the Physical Atlas; and beyond that limit, by laying down Gauss's lines, and drawing the continuation of General Sabine's lines, with a general regard to the forms of Gauss's curves.

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* London, 1853. Neither the scale nor the execution of this chart allows of all the precision which might be desired.

† Berlin, 1857.

The third and most generally important of the magnetic co-ordinates, that of the direction of the horizontal needle, presented some difficulties. The charts of Duperrey (published at Paris in 1836), which appear to have been constructed with very great care, are made the basis; but in filling up the spaces to which direct observations do not extend, it seemed desirable to combine their data with the theoretical lines of Gauss. The chart in Gauss and Weber's Magnetic Atlas,‡ which represents equidistant values of the function $\frac{V}{R}$, gives us a series of lines which are known by theory to be everywhere perpendicular to the direction of the horizontally suspended needle. The lines of Duperrey beyond latitude 60° or 65° were laid down on a polar chart. The Gaussian curves (which ought to be perpendicular to the former) were laid down to the same scale on tracing-paper. The tracing-paper was then tentatively adjusted over Duperrey's lines until an approximation, as good as could be made to perpendicularity, of the two sets of curves was attained, while, moreover, the pole of the direction-curves was adjusted so as very nearly to coincide with Captain Ross's observed position of the magnetic pole. The outstanding deviations of Duperrey's curves from perpendiculars to the Gaussian ovals was then softened by drawing lines slightly differing from the former, but preserving their general form. These latter lines were then laid down in red on the hemisphere. Perhaps, in this process, I have given too much weight to the theoretical as compared to the empirical curves in the Arctic regions. It is well known that Gauss's theory gives a declination in Siberia which differs very sensibly from observation; much more, indeed, than any errors of observation could reasonably occasion. The discrepancy becomes most perceptible when we come to trace the line of No Variation, which, for North Asia, falls in Gauss's charts somewhat to the eastward of what observation warrants.* I have not, however, considered myself entitled to displace the line of "No variation" in any material degree from the position which it occupies in the Russian territory, and which is founded on direct observation. I think it, therefore, right to specify that a certain amount of discordance exists in this region between the position of this line and that of the red curves of the needle's direction.

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* Poggendorff's Annalen, 1833.

† Brit. Assoc. Report for 1837.

‡ Leipzig, 1840.

REVIEWS AND NOTICES OF BOOKS.

The Typical Character of Nature; or, All Nature a Divine Symbol. By Thomas A. G. Balfour, M.D. London: Nisbet & Co. Edinburgh: John Menzies.

This is a work on a theme which is at all times interesting, and especially at the present moment, when society shows, with regard to the relations between the material and spiritual, such an anomalous state of belief. Thus, there are many who repudiate altogether the doctrine of ideal types and of a spirit-world, and who refuse to admit the possibility of design, or of thought, or feeling, in any form, unless a nervous system is given as an antecedent. There are not a few, on the other hand, who claim for themselves the name of spiritualists (but for whom that of spiritists would be more appropriate), who live in the most extraordinary belief as to the nearness and accessibility of the spirit-world to man, and practise upon it in a way that is very strange. And what certainly is not a little curious, there are some in whom these extremes meet, and who, while they reject the doctrine of the supernatural as such, yet believe in the same to a most extravagant extent, provided only they can succeed in construing all to themselves as natural.

Nor is it only popularly and among the half-witted that this contradiction exists and these extremes meet. Among men of science the same field has been entered, and in too many cases with a most unsatisfactory result, inasmuch as many now maintain, as if it were a settled point in the method and logic of science, that every thing and every thought which will bear the name of religious, are

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* See the contrasted charts of Gauss and Erman in the Report of the Committee of Physics of the Royal Society, 1840.

ipso facto excluded from science. In vain, however, do we seek in the writings of such authors for any proof of this. It is, on the contrary, nothing better in the writings of the best of them than a mere dogma or postulate; and for advancing it as such, assuredly there is great want of an adequate sanction. To quote the authority of Bacon on such a point, even though it went a great deal further than it goes, is quite out of place now-a-days. Bacon said, and it is notorious that for many ages previous to his time, reason, without the use of which science cannot exist, had been kept down, and the principle of authority, clothing itself in the garb of religion, had been constantly invoked to settle every question in science as well as in religion. It was therefore natural and expedient that Bacon should do what he could for the emancipation of science. But now that private judgment, even in religion, has been freely allowed its full swing for centuries, the case is widely different; and for the advocates of the free use of *reason* to invoke the *authority* of Bacon to establish the opinion that science and religion have nothing to do with each other, is a paralogism in a twofold point of view. When Newton says, in the general scholium at the end of the *Principia*, "This most beautiful system of the sun, planets, and comets could only proceed from the counsel and dominion of an Intelligent and Powerful Being," he gives utterance to an inference as truly logical and as truly scientific as any to be found in his immortal work. If it be religious as well as scientific, there is no help for it. The truth is, that the distinction between scientific and religious does not exist in pure thought at all. It is wholly conventional. The attempt has been made no doubt, and with much success, to show that the grounds of religion, in so far as reason can reach them, are intensely obscure; and it has been proposed on this account to have nothing to do with them. But then it has also been shown, and that with at least equal cogency, that the grounds of science are equally obscure; and that if mere obscurity in the grounds or subject-matter be a legitimate cause of declining thought, reason is excluded from science no less than from religion.

The fact is, that both science and religion are united by reason in such a way that no man ought to attempt to put them asunder. The same train of thought, logical and continuous throughout, is often purely scientific in its rise, and purely religious in its close; and for any man to say to it in its course of spontaneous development in his mind, "Hitherto shalt thou go, and no further," to arrest it when sacred symbols begin to present themselves as its expression, is merely to do homage to a witless canon, than which there is none that is more arbitrary among the canons of Rome. Far from feeling bound to rest in the merely scientific, the legitimate and spontaneous course of man's free thought, when contemplating and reasoning upon nature, is to find its last word, its haven and its sabbath, in God. Let us then hear our author with acceptance when he so eloquently says, "All nature is invested with a sacredness, when, through the material form, the rays of heavenly glory benignly shine. Everything on earth is but a faint shadow cast by the vast heavenly reality. The kingdoms of this world are but the miniature exhibitions, the fleeting shadows, of the eternal kingdom above. Every exercise of love on earth is but the feeblest reflection of the infinity involved in the expression, 'God is love.' Are judicial functions exercised among men, and is awful justice dispensed with an equal hand? Then here we find the transient image of the spiritual kingdom, for 'justice and judgment are the habitation of God's throne.' . . . Does the blade appear above the earth? immortality is brought to light by this gospel. Or does language express our thoughts, and prove the medium between the seen and the unseen? Then the Incarnate Logos arises before us, the Mediator between God and man. No spiritual condition can exist, however varied, but has its exact image reflected in nature's mirror. So innumerable, indeed, are the instances of this representative character, that we are forced to the conclusion that nature was intended as a typical dispensation," &c.

But it may be said that in this extract, as throughout the volume under review, there is a recognition of miracles, respecting which, assuredly, it may be affirmed with new confidence that science is silent.

Let us see to what extent it can be said, as it so often is, that the idea of miracle is excluded from pure science, and that it attaches not to philosophy at all, but to legend and history only. The argument for this view is the "uniformity of nature." But this, of course, is to beg the question, and to exclude miracles by hypothesis, unless by uniformity of nature be meant some law of reason affirming that nature must be uniform. Now, though this has been argued (and that sometimes very inconsiderately by the friends of miracles), yet, in point of fact, the mind's affirmation of the uniformity of nature is nothing more than a particular application of that general law of mind whereby it universally affirms identity where no difference appears. But this is a law for *thoughts*, and not for *things*. And doubtless, within its own sphere, like all other laws, its authority is paramount and its guidance infallible; for of a thought this is the characteristic, that everything that is in it appears; and therefore there is no room for fallacy in affirming of *thoughts*, that they are identical where no difference appears in them. But with regard to *things* the case is very different. In them there may be much that does not appear; and we have no logical sanction for affirming the uniformity of nature, beyond that strong probability which arises from a very general observation to that effect. Such is all the sanction contained in the so-called

Inductive Principle. But when it has been brought to this, which is its true position, we are compelled to confess, that there is a strong probability on the other hand, also, that the uniformity of nature shall be occasionally diversified by miracle. Thus the general consciousness of mankind affirms that there is in the universe such a thing as liberty or free power. There is in fact such a thing to a certain extent in man himself. But if in man or anywhere else, then assuredly in the Almighty. If, then, creation is to be the symbol or manifestation of the Creator, which is the theme of the volume under review to maintain, we ought to have, and may confidently expect, not only uniformity in nature, as the expression of law and intelligence, but also, on fit occasions, miracles, as the expression of liberty and free power. Without miracle, in fact, creation would be defective as such. It would be wanting in a display of the most wonderful fact in existence, the most central attribute of the being of the Creator—His ever living power and supreme freewill.

But perhaps the entire thesis of our author is denied; perhaps it is said that creation is not a manifestation of the Creator, and that we are not to look to the kingdom of Nature for any types and symbols of the kingdom of Grace at all. This is an opinion in which, we fear, many well-disposed people participate, and in consequence of so doing they look with no hope to science as a revealer of God, and are even jealous of its pretensions. It is certain, however, that such an apprehension is groundless, such an opinion a mistake. If Creation be entitled to that name at all, it must be expressive of the essential attributes of the Creator. It forms our very conception of the relation which exists between cause and effect, that the effect shall be impressed by the characteristics or attributes of the cause which produced it. Whatever may be the facts as to details, and however successful or unsuccessful we may be in discovering the representative and symbolic character of nature, there can be no doubt, if nature be really a creation, that such representation and symbolism exist. We have therefore to thank our author for his volume in which this thesis is maintained, and we cordially recommend his learned work to those who desire to see this view carried out, especially on the Scriptural side.

The Philosophy of Nature; a Systematic Treatise on the Causes and Laws of Natural Phenomena. By HENRY S. BOASE, M.D., F.R.S. and G.S., &c., Honorary Member of the Royal Geological Society of Cornwall. 8vo. London: Longman.

We have here one of those works of which several have appeared of late, which have taken their rise in the belief that experimental science has now been carried so far, and facts accumulated to such an extent, that a systematic conception of the whole economy of nature is possible. And this volume as well as others that we could name have at least this in their favour, that they are the productions of men of very extensive knowledge of nature and philosophical habits of thought, and who have devoted their lives to scientific speculation. It is now upwards of a quarter of a century since our author showed that such was the turn of his mind by the publication of a Geology of Cornwall, his native county, where, if we mistake not, his original design was to practise medicine at Penzance. But having brothers settled as much respected bankers in the flourishing town of Dundee, and seeing an opening for turning his chemical knowledge to account, by securing possession of a large manufactory of that kind in that neighbourhood, he came to the north, where he has now resided for many years, master of his own time, and free to bestow it, as this work shows that he has done, in philosophic thought. And thus the reader of his handsome volume will find himself able to explain and to connect satisfactorily the designation on the title-page (Honorary Member of the Royal Geological Society of Cornwall) with the locality from which the preface is dated (Claverhouse, near Dundee, July 1860).

With regard to the characteristic of the author's system, it is happily laid down by the author himself in an early page; and we prefer quoting his words rather than giving any digest of our own, especially as he presents his theory in a very cogent point of view. "The principle of the proposed system is that of the fundamental idea of Power; which is not to be conceived of as a mere efficiency, but as *a reason-directed force:* a power which is a law unto itself. The *real content* of such a power is its principle of activity or force; the *ideal form* of the same is its reason, whether intuitive or conscious, according to which the force functions; whilst the power itself is the synthesis of *the real and ideal*—an indissoluble union, which is manifested as an essential entity or individual *being.* By abstraction, however, a power may be regarded on either one or the other side of its existence; and according to the stand-point from which it is viewed, the knowledge is said to be real or ideal. Such is the character of a power or reason-directed force. In nature there are a great many powers, each differing from all others in the peculiar attribute of its force, and in the special law by which the operations of its force are regulated. But the grand and distinguishing character of natural powers is, that they are always associated together in pairs or dualisms: there is no such thing in nature as a single insulated power; such a power can only be an unconditioned or absolute being. On this twofold constitution of all natural bodies physical phenomena depend; for it renders action and reaction possible, without which they could not function. And not only so, but in each dualism the powers are not only coexistent, but also directly opposite in their attributes; and it is to this antagonism of forces that the conditioned character of nature must be attributed; for the constituent powers of natural bodies mutually control and limit each other's energies." (P. 10.)

Such is the profound yet perspicuous paragraph in which the author first announces the general principle of his system; yet we must not leave it to stand by itself, otherwise the reader might be apt to infer that we have in our author's views only a scientific development of Spinozism on the physical side. That such an inference, however, would be a complete mistake, the following paragraph renders plain:—"Natural beings, as we have already stated, are dualisms of powers. We can, however, conceive of an infinite and absolute power, by positing it as the negation of a natural conditioned power; and by contemplating the transcendent wisdom manifested in the operations of finite powers, we may infer that such a power is omniscient as well as omnipotent. Thus far, by the light of nature, man may arrive at the conception of a Supreme power: but if he seeks to know the relation in which he in common with all other natural subordinate powers bears to the Supreme, he desires knowledge which nature cannot impart; he can therefore only obtain it through some supernatural channel. Revelation has taught us that Absolute Being, the Great I Am or Deity, is such a power as we are considering; a Reason-directed Force to whom all things are possible according to the counsel of His will, which is the perfection of wisdom . . . . And as one star differs from another in glory, so cosmical beings rise one above another in dignity till they culminate in man, whose attributes are a microcosm comprehending all others, together with its distinguishing mark of conscious reason. The natural universal is distinguished from the *summum genus* of ontology by its conditioned or finite character; and from the collateral branch of the conditioned, the angelic host, in having material instead of spiritual bodies; or, in other words, the natural and the supernatural are distinct branches of conditioned beings, subordinate to, but not forming part of, the Absolute or Supreme Being." (P. 19.)

Our author having stated the general grounds of his system, proceeds to comment, though very shortly, on the unsatisfactory character of the German Idealism, the Positivism of Comte, and the doctrine of the identity and convertibility of physical forces as advocated by Grove. It is to the last that his own views come nearest; and we cannot help thinking that it is rather a case of contact than of collision. Both maintain the correlation of the physical forces and their reciprocal dependence: Grove, that they are capable of reciprocal transmutation (and that of course under the universal law of action and reaction); our author, that they are co-existent and opposite, but one member of the dualism often masked or latent. There is however, it must be confessed, in our author's views, something singular in that he ignores inertia as he does. It is commonly regarded as the very characteristic of matter. But our author, on coming to particulars, and in answer to the question, What is matter? (p. 37), says: "The whole tendency of modern science points to the probability that natural forces are not only the causes of phenomena, but that they are in truth the very phenomena themselves; that is, that all natural bodies are compositions of forces." And this view, which he presents in connection with the name of Boscovich as its great originator, he advocates—laying down as his first dualism the following formula:

Attraction + Repulsion = Matter.

He then proceeds to inquire whether there be any pure matter or merely physical substance in nature, and he decides in favour of the universal ether. This he also regards as the material out of which the molecules of bodies are composed by apposition of the ethereal elements. But these molecules he places in a different category from pure matter, or merely physical substance, inasmuch as they are chemical agents also, and as such are constituted by a higher dualism. Above this, again, there is another dualism, of which organism and instinct are the products; and above that another still, to which intuition and volition belong; the whole being discussed in a very masterly manner, although we confess with a frequent obscurity (perhaps inevitable), which leaves the reader too often without any distinct conception of the phenomena which the author has in his mind, and which therefore forbids an attempt at analysis.

His first specific dualism is more amply illustrated. And here we find solar repulsion, which formed the theme of such an animated discussion at the Institute during the time of Donati's comet, fully inaugurated in couples with gravitation, the dualism answerable to astronomy being

Gravity + Centrifugy = Celestial bodies or stars.

But on his argument we cannot enter here, and must content ourselves with commending this work to such of our readers as are bestowing thought upon the *prima philosophia;* yet not without also confessing our apprehension that in some at least of our author's dualisms there is more that is subjective than objective, and a product of the mind in its two grand co-ordinate phases of analysis and synthesis, rather than of nature

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Species not Transmutable, nor the Result of Secondary Causes; being a Critical Examination of Mr Darwin's Work entitled "Origin and Variation of Species." By C. R. Bree, M.D., F.L.S., Physician to the Essex and Colchester Hospital, author of "The Birds of Europe not observed in the British Isles." 12mo. Groombridge and Sons. London, 1860.

The subject of the origin of species has called forth many able and valuable publications, and has excited, of late years, a great deal of interest in the public mind. The early views of Lamarck were followed by those of the author of the Vestiges, and these have been superseded by Mr Darwin's speculations. The work of the last-named writer is characterised by much ability, and, coming from a naturalist of undoubted eminence, it has attracted special attention. Darwin's inference is, that probably all the organic beings which have ever lived on this earth have descended from one primordial form, into which life was first breathed. This form has undergone variations during unknown millions of years; and by a constant *divergence*, and, at the same time, *correlation of growth*, all the species of the present as well as of former epochs have been developed. The species are now undergoing transformations, as their predecessors have done, and are subjected to a constant *struggle for life*, in the course of which weaker and less perfect forms disappear, while the stronger and more highly developed continue to live. The changes produced depend on supposed laws of variation which have reference to the wellbeing of the individual. There are no creations, but constant transformations or transmutations; no rapid transitions from one condition of organic life to another, but a gradual and progressive development, which gives rise to more and more perfect forms. Such are the ideas of Mr Darwin. He has brought some interesting facts in support of his views, and has taken a comprehensive grasp of nature in all its phases. He attempts to show that species are not fixed, but mutable; that the formula of Mr Wallace is correct, that "every species has come into existence coincident both in time and space with pre-existing closely allied species;" and that there are transition forms or gradations which connect all organic beings, and which point to hereditary descent from one common stock of unknown antiquity. Some there are, such as Lyell, Hooker, Draper, and Huxley, who fully support Darwin, and believe that the explanation he gives is the only one which satisfies the demands of the naturalist, and reconciles the phenomena presented by the organic world in all its phases, past and present. Others, such as Asa Gray and Pictet, adopt the theory to a certain extent, although they think it may be carried too far. Pictet thinks that the theory accords well with the great facts of comparative anatomy and zoology; that it explains unity of composition in organisms, as well as rudimentary organs, and the natural series of genera and species; but he rejects Darwin's conclusions, and thinks that he has made unsound deductions, not warranted by the facts adduced. Asa Gray, a sound and judicious botanist, and whom no one will suspect of favouring materialistic or unscriptural opinions, says, in the "Annals of Natural History" for November 1860, "We have contemplated quite long enough the general presumption in favour of a hypothesis of the derivation of species. We cannot forget, however, while for the moment we overlook, the formidable difficulties which all hypotheses of this class have to encounter, and the serious complications which they seem to involve. We feel, moreover, that Darwin's particular hypothesis is exposed to some special objections. It requires no small strength of nerve steadily to conceive, not only of the diversification but of the formation of the organ of an animal, through cumulative variation and natural selection. Think of such an organ as the eye, that most perfect of optical instruments, as so produced in the lower animals, and perfected in the higher! A friend of ours, who accepts the new doctrine, confesses that for a long while a cold chill came over him whenever he thought of the eye. He has at length got over that stage of the complaint, and is now in the furor of belief, perchance to be succeeded by the sweating stage, during which sundry peccant humours may be eliminated from the system. For ourselves, we dread the chill, and have some misgivings about the consequences of the reaction. We find ourselves in the singular position acknowledged by Pictet—that is, confronted with a theory which, although it can really explain much, seems inadequate to the heavy task it so boldly assumes, but which, nevertheless, appears better fitted than any other that has been broached to explain (if it be possible to explain) somewhat of the manner in which organised beings may have arisen and succeeded each other. In this dilemma we take advantage of Mr Darwin's candid admission, that he by no means expects to convince old and experienced people, whose minds are stocked with a multitude of facts, all viewed, during a long course of years, from the old point of view. This is nearly our case."

Again, there is a third set of naturalists, such as Harvey, Brodie, Beale, Jardine, and Murray, who are thoroughly opposed to Darwinian views; while there are cautious naturalists, who have not yet committed themselves to any opinion on the matter.

Every one must feel that, when we come to consider man, the difficulties are great. We have in his case a record of Truth to which we can appeal; and although there can be no doubt that Scripture does not teach the facts of science, and that short-sighted man is apt to give false interpretations of what he reads there, still there are statements as to man's creation, and the ultimate destiny of the earth, which appear to be wholly irreconcilable with Darwin's views, carried out to their full extent. Grant that the statements in Genesis may be variously explained, and that they may appear to some to be at variance with the general views of geology at the present day, still there are distinct and clear enunciations which cannot be set aside. The Bible says that God "created man in his own image;" that He "formed him of the dust of the earth, and breathed into his nostrils the breath of life, and man became a living soul;" but the theory of Darwin, as we understand it, says that man was not created, but developed from previous organic forms, which have passed through all gradations, from the protozoon to the gorilla. Here is a stumbling-block which we cannot easily get over; and we are certainly entitled to an explanation. Scientific men are not at liberty to ignore the statements of Scripture. They are bound to examine them, and to show, at all events, that there is no incompatibility between its statements and their hypotheses. The true facts of science will never be found to be in opposition to the Bible; both must necessarily be in perfect harmony.

In all these theories of progressive development, the Creator has been made to retire from His works. He may have given origin to a cell or organic form millions of years ago, but He merely set the mechanism agoing, and left it to work out its own improvement according to what are called *laws*. What are these laws, and how are they executed? A sovereign may make a law, but unless it is carried out by some one, what is the use of it? It has no potency in itself, and requires a living agent to give it effect. Laws in nature are the expression of the Divine mind, and they are constantly applied and executed by the Creator himself. No law can exist without His unwearied superintendence and direction. It is vain to attempt to hide the Creator by talking of the laws of nature. These laws must be viewed as being upheld every moment by His all-wise and all-powerful preserving and governing of all His creatures and all their actions.

Various able articles have been written in opposition to Darwin's views, by naturalists of note. These have been published in the Proceedings of Societies and in Scientific Journals, both in Britain and in America. At the same time, as has been stated, Mr Darwin has supporters of high reputation in science. The present work is one of a somewhat popular character. It is written by a medical graduate, who has distinguished himself by his ornithological writings, and who has devoted much attention to natural science. He takes up the various points of Mr Darwin's argument, and treats them in an interesting, and upon the whole convincing manner. He examines the general bearings of the work, and shows its dangerous tendencies. The geological data receive consideration, and the absence of anything in Palæontology to aid Darwin's views, is dwelt upon as being most damaging to his theories. In speaking of the supposed transmutations, Dr Bree says :—" There is a great deal said of such arguments as are to be derived from the ready belief, that a flying-fish might be converted by *natural selection* into a bird. How the flying lemur might have its membranous appendage changed into the wing of a bat. How the eye of the eagle might have been *modified* and *varied* and developed from the black pigmentary spot of the crustacean; or how a bear, swimming about with open mouth to catch flies, might have been transmuted in due course into a mighty whale. But when we ask for proof,—for facts to argue upon,—for reasons for believing, —for the scientific basis of a rational induction,—alas, we get nothing but the doctrine of *natural selection*, of *modification of form*, of *divergence of character*, of *correlation of growth*, or we are coolly referred, with inimitable equanimity, to that dark unfathomable abyss, the *imperfection of the geological record*. That domestic animals of the same genus will modify, no one ever doubted. That climate, habit, differences in food, and careful and judicious crossing, will alter the races of animals, as to certain unimportant points of structure, is a truth which no naturalist ever denied. But the pigeon, reared by the fancier, is still a pigeon; the short-horned ox and the Devon are still most unmistakably bovine; the racer and the cart-horse still proclaim their brotherhood; the greyhound and the spaniel are still dogs" (pages 8 and 9). These are important statements. The variations produced by domestication, &c., do not go the length of changing the true characteristics of the type.

In the case of plants, numerous varieties have been produced by cultivation and by other causes. Some of these have, it is true, been erroneously exalted into species, and thus great confusion has often been produced. The following observations have recently been made on the species of species-makers by Mr Carriére, which have been translated in the "Gardener's Chronicle" for December 8, 1860. The specific type of different beings is extensible, and these again may be variously altered. Species offer a certain amount of resistance to change, which once overcome, each individual of which it is composed becomes far more susceptible of being moulded and of undergoing changes. Some plants have been long cultivated without change; others show a manifest tendency to vary. In some cases these changes are induced by simply sowing the seeds in highly cultivated soil, while in others there is need of artificial fertilizing. This fertilization has not been effected between different species, as is too often supposed, but between half-breds, *i.e.*, between different varieties of the same species. All individuals thus obtained will breed to any extent. There has been a too ready admission of so-called new species. If all the pretended novelties were carefully examined, the number of the species would be reduced by one-half or more, as would also the genera. This excessive multiplication of species is well seen in such genera as Rubus and Hieracium. In Linnæus's time there were two species of brambles, *Rubus fruticosus*, and *R. cæsius*. But of these Grenier and Godron have made 22 species; Boreau has made 54, and a German bramble-monger called Müller has made 238. The species of Hieracium have been so multiplied, that they can only be distinguished by the names or numbers stuck at their sides in herbaria or botanic gardens. If these names or numbers happen to be lost, it is in most cases quite impossible to identify such species, the trifling characters of which can only be compared to those of the persons who established them. Some idea may be formed of the confusion that at presents exists in this genus, when we recollect that the *Hieracium murorum* and *sylvaticum* of authors have naturally brought forth 60 new species. The true observer of nature will bring these varieties, within the compass of the species to which they rightfully belong. These variations of species are limited. They have a certain range beyond which they do not extend. Cultivated varieties, when left to grow wild, return to the original stock whence they came. Darwin has not disproved this by his statements. Neither the vegetation of the present day nor that of bygone epochs of the world's history support his views as to the origin of species. He cannot produce intermediate transition forms, even with the help of countless ages of geological formations. Dr Bree thinks that in every great period of geological history, the world has been peopled by creatures adapted to its physical and climatic condition, and that there is no evidence to show that this has not been done by distinct acts of special creation, by Him whose wisdom our finite minds are too apt to interpret and criticise.

The denial of *per saltum* changes seems to savour much of the views of the scoffers mentioned by St Peter, who thought that all things had gone on from the beginning without catastrophes of any kind, and who did not believe that the present world was to be finally burnt up and a new earth formed. Darwin assumes that "no cataclysm has desolated the whole world, and that we may look with some confidence to a secure future of equally in-appreciable length," in which, "judging from the past, we may infer safely that not one living species will transmit its unaltered likeness to a distant futurity." On the dogma, *natura non facit saltum,* Dr Bree makes some valuable remarks; and in pages 52 to 61 he gives a graphic and amusing description of the appearance of various organic tribes on the earth when viewed from Darwin's stand-point. In this sketch he attempts to bring out the fallacies of the system, and the crudeness and untenableness of the theory.

Another point on which Dr Bree dwells, is the implied imperfection of organic beings as regards their functions, and the necessity for a gradual improvement by their own efforts. While Darwin speaks of instinct and adaptation as being at first imperfect, and as being gradually improved by individual selection, Dr Bree shows that, as regards the instincts of animals, the formation of the various organs, the minute structure of the body, and the microscopical character of the fluids—all is adjusted in a most perfect and complete manner, indicating creative design and not mere chance variations by natural selection, as it is called.

Dr Bree treats the subject in a clear, distinct, and popular manner, and brings strong arguments of his own and of others to bear on Mr Darwin's theory. We recommend the book to the perusal of all who take an interest in natural science.

Geological Treatise on the District of Cleveland, in North Yorkshire; its Ferruginous Deposits, Lias, and Oolites; with some Observations on Ironstone Mining. By JOSEPH BEWICK, author of "Remarks on the Ore and Ironstone of Rosedale Abbey." 8vo, pp. 194. London: Weale. Newcastle: Reid. 1861.

In these times of *neck-and-neck* competitions in commercial enterprise, and *hand-to-hand* conflicts for political ascendency, wherein the ingenuity of man is taxed to the very confines of his intellectual as well as of his physical capabilities, whether for the attainment of individual prosperity or for the gratification of national ambition,—a question of no mean importance suggests itself as to the durability of the resources at his command in the one case, and, in the other, of their superiority both in kind and degree over those of his antagonists. The political importance of a nation, it may be assumed, depends less upon the numerical strength of her forces *per se,* than upon the extent of her natural resources, and the amount of fertilizing genius which she can command to operate economically upon them; and, in truth, she owes her supremacy not more to the strategic qualifications of her generals than to the patient investigations of her philosophers. What proportion the mineral products of this country bear to the sum of its wealth it is not easy to divine. Agriculture in its progress has denuded the surface of its wood; science in return has discovered a wealth of coal and iron. Railways and shipping draw largely on both of these; and, if we mistake not, the latter will make a still greater demand on the iron field when—as we undoubtedly shall have to do—we change the time-honoured epithet of our national bulwarks, and call them no longer our "wooden" but our "iron walls." However fascinating may be the pursuit of the speculative science of geology through the fiercely contested theories of the Neptunists and Plutonists, or through the still more remote doctrines of the ancient Greeks and Romans, back even to very chaos itself; it has yet another function, and one of far greater practical utility, by which, in the capacity of pioneer, it guides our labours in the useful and decorative arts; pointing here to marble or granite for the sculptor, there to materials for the builder; and again, directing the engineer or the miner—thereby subordinating external nature, and adapting her more closely to the moral, intellectual, and physical condition of the great human family.

These somewhat disconnected but pertinent reflections have been suggested by the title of the book before us. Before proceeding to remark on the merits of the work, however, it will be important to the reader to have a more accurate knowledge of the geographical position of the district treated of than the author has deemed necessary to supply. Cleveland, Ord tells us, forms part of the North Riding of Yorkshire, and is "bounded towards the north by the Tees and German Ocean; by Mulgrave Castle and Thordisa on the east; by Grosmont Bridge, part of Wheledale Moor, Skinner Howe, Rolles Cross, and Westerdale to the south; then ascending the summits of Ingleby, Broughton, Carlton Banks, and Arncliffe, it includes Appleton-upon-Wick as an extreme boundary on the west. It is forty miles long and eighteen broad." From the chain of lofty cliffs forming its limits at the German Ocean to its opposite boundary, this tract of land presents a series of peculiar and characteristic features, the most prominent of which are those "immense ranges of majestic hills," of which the author treats. The contrast of its wild and wide-spread moors, with its nooks of verdant pasturage; its dales, its rivers, and its woods, render this locality a pleasant retreat to every lover of the beauties and sublimity of nature.

The work we speak of is divided into six distinct parts or treatises, of which the first and second comprehend a geological delineation of the district, the former describing the consecutive strata of the formation, the latter having particular reference to the ironstone seams. In the third treatise the author gives a practical explication of ironstone mining as pursued in the mines under his own direction, together with some remarks on the necessity of a careful and thorough ventilation. The fourth section contains some practical suggestions for the improvement of the port and harbour of Whitby. Fifthly, we have a paper on the coal deposits. Sixthly, a brief paper on the "probability of reaching the Coal-measures under the Lias and other formations in North Yorkshire and Cleveland." And, lastly, a short appendix containing a "tabular synopsis of the strata forming the Oolitic and Lias groups in the vales of Eskdale, Goathland, Newtondale, and Pickering." Hereafter we may perhaps take occasion to offer some remarks upon the purely scientific part of the work, but at present we confine ourselves to the subject in its practical bearing, which is undoubtedly one of great national importance. The ironstone of Cleveland (originally Cliffland) had for many years been known to the ironmasters in the north, but owing to some mismanagement in selecting the specimen cargoes despatched for their approval, its value was not duly appreciated; and it was not until the year 1837 that it met with favourable acceptance. In 1839, Mr Bewick was called to Grosmont for the purpose of conducting the mining operations about to be commenced by Mrs Clark in her own estate. At the same period the "Whitby Stone Company" sunk their first shaft, likewise at Grosmont, whence the mineral was transmitted by the Whitby and Pickering Railway—"one of the early undertakings of the elder and illustrious Stephenson"—to the port of Whitby. The cautious manner in which Stephenson gave his evidence before the House, when questioned as to the probable speed of his locomotive, is worthy of remembrance by all who are connected with new undertakings; and, indeed, the absence of such caution on the part of the author of "Cleveland" might have been productive of serious consequences.

"After having completed our arrangements for a supply, the managing partner of the firm expressed a hope that we had not undertaken to supply a quantity of ironstone we were unable to work—viz., 10,000 tons per annum; in answer to which we observed that, before the year was over, we had little doubt of being able to raise three or four times that quantity. The observation thus made subjected us to a severe rebuke from that gentleman, accompanied by a well-intentioned admonition, to the effect that nothing was more disadvantageous to a young man than to make unguarded and exaggerated statements. We endeavoured to assure him that there was, we believed, no exaggeration in the statement we had made, in proof of which we had only to mention further that we had ascertained, from careful experiments, that two seams of ironstone in Mrs Clark's royalty would produce the unheard of quantity of 20,000 tons per acre. This announcement appeared only to make things worse, and he expressed very grave doubts of our being able to fulfil even the engagement we had entered into, being apparently convinced that we were wrong in our calculations, and were labouring under a false impression."—p. 20.

The correctness of his estimate, however, was subsequently placed beyond doubt; for, in 1846, he says—"This contract increased Mrs Clark's vend to 30,000 tons per annum."

The prosperous state to which the iron trade had attained in 1840-41 was followed, in 1842-3, by a more than corresponding depression, when pig iron was quoted so low as 35s. per ton. This, together with the discovery of the "Black Band" in Scotland nearly drove the Cleveland mineral out of the market. By mutual agreement, shipments were suspended at Whitby for two years, after which the iron trade revived, the Black Band disappointed many who had speculated largely in it, and Cleveland ironstone was again in demand. In 1844-5 pig iron was quoted at £6 per ton, and from that time until now the shipments from Whitby have been very considerable. Cleveland stone is not used alone, as it has been found to produce "what is termed, cold, short iron,—viz., iron wanting toughness; which fault is corrected by an admixture of hematite ore, obtained from Cumberland, a comparatively small proportion of which suffices."

The annual produce of the three mines at Grosmont is nearly as follows:—

	Tons.
The Whitby Stone Company, about . .	30,000
Mrs Clark	30,000
The Birtley Iron Company	10,000
Total	70,000

The development of the ironstone workings at Grosmont is checked by the difficulties of transit to the furnaces; the only method of effecting this at present being by means of small vessels sailing from Whitby; and the author does not hesitate to say, that if the harbour were improved, so as to admit a larger class of shipping, the annual production of ironstone at Grosmont would reach ten times its present amount. A line of railway (the North Yorkshire and Cleveland), which is now in process of construction through the very centre of the iron districts of Cleveland will, it is anticipated, very shortly connect Grosmont with the northern coal-fields; and other arrangements contemplated by the North Eastern Railway Company will materially facilitate mining operations throughout the entire locality. The author, however, attaches great importance to improvements in Whitby harbour.

The extent of the mineral field of Cleveland we gather from the author's opinion to be somewhat as follows:—Estimating the superficial area over which the mineral may be expected to occur, in sufficient quantity to make it worth working, at thirty miles by sixteen, and deducting from this, to compensate for the denudations which exist both in the valleys and in the outcrops, sixty square miles, we have—

$$30 \times 16 - 60 = 420 \text{ square miles.}$$

Then, if the average yield be (as the author believes it will) 20,000 tons per acre, and the loss of stone in working it be calculated at 2066 tons, we have a net produce of 17,934 tons per acre, which, multiplied by 640 for miles, and this again by 420 as the number of square miles over which the mineral is supposed to extend, gives us, as the gross quantity of ironstone obtainable from the Cleveland iron field, the enormous mass of 4,820,659,200 tons. "We will assume," says Mr Bewick, "that 200 blast furnaces, making each 200 tons of pig iron weekly, for fifty full weeks in the year, draw their supplies from this source; say then 200 furnaces × 200 tons × 50 weeks × $3\frac{1}{2}$ tons to the ton of pig = 7,000,000 of ironstone consumed per annum. Next, 4,820,659,200 ÷ 7,000,000 = 680 years of time during which the main seam of ironstone may be supposed to meet the assumed demand. Two hundred blast furnaces is a large number, but when it is considered that this is the cheapest iron-making district in the United Kingdom—indeed in the world—and that so many as sixty-eight furnaces have been erected for the use of the Cleveland ironstone within the last sixteen years—fifty of which have, in fact, been constructed within the last ten or twelve years—we are disposed to think that, had we taken double the number for our calculation, we should not have outstepped probability. It must not be overlooked, however, that the above estimates refer solely to the main seam, leaving the top seam as well as the upper and lower oolitic ironstone deposits intact. It would be difficult to estimate approximately, by a course of figures, what might be likely to be drawn therefrom, but we feel well assured that after the main seam workings have progressed for any considerable length of time, say a century, or perhaps two, these will be brought into play; and our decided impression is, that the above 680 years' duration may be extended to 800 or 900 years." —P. 123.

We may perhaps, in a future number, continue our remarks. At present we must take our leave of Mr Bewick's very interesting volume at this point. The illustrations, consisting of a large geological map of the district, with two elaborate sections, and plans of mining operations, are neatly executed; and the book throughout evidences considerable taste on the part of the publisher. The experience which the author has had in mining pursuits generally, and especially his intimate acquaintance with the district of Cleveland, gives a force to his suggestions which practical men would do well to ponder. His object appears to have been to render his work essentially practical, and he has succeeded. He writes as one conscious that, by imparting his knowledge in a plain and straightforward manner, he is rendering his country a service. There is in his language an earnestness which arrests the attention and commands respect, and withal an absence of that controversial petulance which is so frequently observable in essays of this kind.

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Monday, 3d December 1860.—His Grace the DUKE OF ARGYLL, President, delivered an Opening Address. The following is an extract from the address:—

One of the duties which devolve upon me to-night, and one with which it is perhaps best that I should begin, is the melancholy duty of recording the names of those whom death has separated from our fellowship during the Session 1859–60. They are as follows:—

William Alexander, Esq.
Dr James Andrew.
Rt. Hon. Lord Arbuthnot.
Sir T. M. Brisbane, Bart.
Dr George Buist.
Hon. Mountstuart Elphinstone.
Sir James Forrest, Bart.
Sir John Hall, Bart.
John Lizars, Esq.
Sir John Melville.
Dr George Wilson.

Sir T. Brisbane was descended from an ancient and honourable family, whose representative occupied the high place of Chancellor of Scotland in the middle of the fourteenth century. He was born in 1773, and entered the army in 1789. A contemporary of Arthur Wellesley, he was early thrown into his society in Ireland; and thus began a friendship which was cemented by a close companionship in arms, and lasted to the end of the great captain's life. Sir Thomas Brisbane's active military service began in 1793, in which year his regiment formed part of the Duke of York's expedition to Holland. From 1795 to 1798 he was engaged in the various affairs by which the West India Islands were successively reduced. It was during his voyage out in 1795, that having been in imminent danger of shipwreck in a collier transport, from the ignorance of the captain, he was first led to direct his attention to astronomical observation.

Having acquired by purchase in 1799 the lieutenant-colonelcy of the 69th regiment, he returned to England, but finding that that regiment had meanwhile been sent to Jamaica, he was obliged to repair to that island in the following year.

Sir Thomas Brisbane's health having suffered severely from the effects of climate, he was obliged to retire on half-pay, when in 1804 his regiment was ordered to India. But in 1810 he was appointed to the staff at Canterbury as assistant adjutant-general; and on the army going out to Portugal, he applied for an appointment under his old friend Sir Arthur Wellesley. In 1812 he secured this great object of his ambition, and as brigadier-general joined the head-quarters of the army then at Coimbra.

Sir Thomas remained in this high command throughout the remainder of the Peninsular war, and for his distinguished services, especially at the battle of Orthes on the 27th February 1814, he had the honour of receiving by name the thanks of the British Parliament. At the close of the Peninsular war, Sir Thomas Brisbane was selected for the command of one of the brigades which were then sent out to Canada, where he used his influence in putting an end to the barbarous practices too often resorted to by both parties in the unfortunate war with America. The escape of Napoleon from Elba recalled Sir Thomas in haste to Europe, where, however, he arrived too late to take part in the final triumph of his great Commander.

There is one curious incident of this period of Sir T. Brisbane's life which is specially interesting to us. It is well known how intense was the feeling of bitterness against the French government and people roused in the German nations by the cruel humiliation they had all successively undergone from the successful tyranny of Napoleon. Some of the public buildings of Paris, commemorative of his victories, were saved only by the personal interference of the Duke of Wellington. It appears that another, the abode of no less celebrated a body than the Institute of France, was saved through the appropriate agency of Sir T. Brisbane. The claim thus established on the favour of the most distinguished scientific society in the world, in addition to that founded on his own acquirements and pursuits, was speedily acknowledged. On the motion of Bouvard, the French astronomer, Sir Thomas Brisbane's name was added, by an unanimous vote, to that roll of membership, which affords, and has long afforded, one of the most valued honours attainable by the successful cultivators of science.

In 1820 the continued favour of the Duke of Wellington procured for him the governorship of the important colony of New South Wales. It was this command at the Antipodes which enabled Sir Thomas to render to astronomical science those new and important services which procured for him, four years after his return, the gold medal of the Royal Astronomical Society of London. He established, and maintained entirely at his own expense, the now celebrated observatory at Paramatta. So early as 1808, when his health had compelled him to retire for a time from active service, he had erected an observatory at Brisbane, his native place; and some of the instruments procured for this establishment were the first with which observations were begun at the Antipodes.

Sir T. Brisbane was elected an F.R.S.E. in 1811, but in consequence of his various military appointments abroad, he did not personally take much part in its proceedings until about 1826, when his name appears on the list of the Council. In 1832 he succeeded Sir Walter Scott as President of the Society, an honour which he fully appreciated to the very last. A certain simplicity of character, combined with a dignity and courtesy which peculiarly became him, made him deservedly and universally popular among the Fellows. The perfect disinterestedness with which he devoted himself to science, added to this favourable impression a feeling of sincere respect. He was lavish of money when any scientific object was in view. Many an unfriended but ingenious person has been encouraged by his liberality, which only erred sometimes on the side of being too indiscriminate. About seventeen years ago, having fallen heir to a considerable property, his first thought was how to spend it best for the advancement of his favourite sciences. After consultation with one or two persons on whose judgment he relied, he determined on erecting the magnetical and meteorological observatory at Makerstoun, and on supporting the needful staff of observers at his own expense. The valuable observations which were made there, most ably superintended, for the most part, by Mr J. Allan Broun, were afterwards printed at great length in the Transactions of the Society, at the joint expense of the Society and of Sir Thomas himself.

Among the Fellows of this Society whom we have lost during the present year, there is another whose name I cannot pass by in silence, or with mere mention only: I mean the name of Mountstuart Elphinstone. In all probability there are few members of this Society now present to whom this distinguished man was personally known: because the greater part of his life was spent in India, and the remainder of it in very close retirement. But his name is familiar to all of us as one of the most eminent among those whose courage and ability have built up the colossal fabric of our Indian empire. So far as active service is concerned, he was a yet earlier companion in arms of the great Duke than Sir Thomas Brisbane. Alternately acting as soldier and civilian, as in the earlier days of the "Company" all her great servants occasionally did, he took an active part in the campaign which founded the fame of Arthur Wellesley. Mountstuart Elphinstone has, however, a higher claim on the grateful recollection of his country. When war had done its work, and the time had come for governing the people who had been conquered, his powers of administration were as conspicuous as his courage in the field. By the universal consent of all who know the history of our Indian empire, he is regarded as one of the very greatest of those whose wisdom and virtue have tended to reconcile its people to British rule, and have founded those traditions of government which, modified more or less by the progress of events, must continue in the main to be the guide, not only of us in India, but of all nations who undertake the difficult and responsible duty of ruling other nations, different from themselves in race, language, and religion.

George Buist, LL.D., F.R.SS. L. and E., and G.S., another recently deceased fellow of the Society, was born at Tannadice in the year 1805. His father having been minister of that parish, which is in the presbytery of Forfar, Dr Buist was educated at St Andrews, and studied divinity for the purpose of becoming a minister of the Church of Scotland; but, though licensed to be a preacher, he never was ordained as a minister of the Church. He cultivated with assiduity the study of science, especially in its bearings on natural history and geology, founded a provincial society for its prosecution, and gained the prize offered by the Highland and Agricultural Society for an account of the Geology of Perthshire, which is published in the Transactions of that Society. During his residence in India he contributed many papers of interest to the scientific societies of that country. In addition to these, he also published papers of interest on its antiquities and history. Many important public works enjoyed much benefit from his active co-operation. Among these may be mentioned the establishment at Bombay of an industrial school for natives, wherein a knowledge of British manufactures was taught, and which led the way to similar industrial institutions for the other presidencies. Dr Buist died on a voyage to Calcutta, on the 1st day of October last.

I wish I were capable of presenting to the Society anything like a really useful review of the progress of science during the year which is about to close. This I cannot pretend to do; but perhaps I may be allowed to direct your attention to one or two subjects to which that progress has been important.

To begin with our own country, and with an investigation the importance and interest of which has been acknowledged by the Society in the grant of the Brisbane medal,—I have reason to believe that Sir Roderick Murchison has been prosecuting with farther success his examination and reclassification of the more ancient rocks of Scotland. The clue afforded some years ago by the discovery of Mr Peach, that the limestones of Duirness in Sutherland contained fossils of the Lower Silurian age, has been followed up by our distinguished countryman Sir Roderick, with his usual indefatigable perseverance, and his usual sagacity of interpretation. The result of his last researches goes far to extend the light already thrown on the rocks of Sutherland and Ross to the vast series of micaceous and quartzose strata which constitute the great bulk of the Western Highlands in the counties of Argyll and Inverness. And I think it a circumstance worthy of mention, that some years before the discovery of the Sutherland fossils, and before, therefore, any clue from organic remains had been afforded, Sir Roderick Murchison had suspected that the whole series of metamorphic slates in the district to which I refer were nothing more nor less than altered strata of Silurian age. He expressed that suspicion strongly to myself in 1850, when I had an opportunity of pointing out to him some of the more characteristic beds in the neighbourhood of Inveraray. During this last summer and autumn, he has traced the upward series of rocks from what he calls the fundamental gneiss in Sutherland and Lewis, southward to the islands of Islay and Jura, and by a close examination of the stratigraphical relations, is now prepared to furnish proof of the truth of the conclusion to which by a species of instinct he had been led before. In one of the facts upon which this determination rests, I think I can venture, from personal observation, to confirm his argument. The term gneiss had been correctly applied by M'Culloch to the fundamental rock of the outer Hebrides, a rock which reappears in great mass on the south-west coast of Sutherland. But unfortunately he applied the same term to other rocks, which are now proved to overlie beds containing Lower Silurian fossils. He thus confounded strata which are separated by immense ages from each other. Now, Sir Roderick Murchison has pointed out the essential differences of lithological character which distinguish the fundamental gneiss from all the rocks of the overlying series. When these differences are once pointed out, it is impossible to mistake the two. The fundamental gneiss is distinguished by the predominance of hornblende, so thickly laid, generally in lines parallel to the stratification, as frequently to render the stone almost black. The felspar and mica are generally found in large separate crystals and plates; and it is not unfrequently intersected by veins and masses in which the same mineral constituents are more perfectly mixed in the form of granite.

To this rock, which is largely developed in our North American possessions, where also it is succeeded by a very similar series of overlying deposits, the term "Lawrentian" has been applied by Sir William Logan.

This term Sir Roderick Murchison proposes to retain for the oldest stratified rock yet known in the world. Upon this fundamental Lawrentian gneiss are piled the vast series of Cambrian strata which constitute the great mass, and sometimes the whole, of the most striking mountain-forms on the west coast of Sutherland and Ross. These strata are estimated by Murchison to measure some ten or twelve thousand feet in thickness. Resting again unconformably upon these Cambrian beds, and capping with their white quartzites many of the mountains, the true Silurian rocks appear, distinguished—mainly in the limestone bands, but also, though more rarely, in the quartzites—by orthoceratites, and other characteristic fossils. Intercalated among these, and therefore having their relative age clearly determined, occur those other more crystalline and metamorphic strata to which the same term gneiss had been also unfortunately applied. But no two rocks can be more different than those overlying rocks from the fundamental gneiss. I have never seen in any part of the South-west Highlands, among the mountains which M'Culloch assigns to gneiss, any rock approaching in character to the gneiss of the Hebrides and of the north. The question, however, will, I have every reason to believe, be finally settled by the proofs which are about to be brought before the Geological Society. Sir R. Murchison has found that the islands of Islay and Jura present perfect repetitions of the phenomena of Sutherland, and that the quartz rocks and limestones of Silurian age are superposed conformably and without a break by the micaceous and chloritic series which occupies such large tracts on the opposite mainland, and which, folding over a little south of Loch Tay, and clasping round Schiehallion, again rises up to the north of Loch Rannoch, and allows the lower quartzites and limestones to reappear. Very curious questions arise as to the causes of the metamorphic action which has so completely altered the structure of beds lying over others which remain comparatively unaffected. Some geologists have been inclined to deny the existence of true stratification in the micaceous chloritic schists of the South-west Highlands, and to assign the appearances to lamination or slaty cleavage. I must say I agree entirely in the view taken by Sir R. Murchison, that this doctrine is wholly untenable. Indeed, I can with difficulty suppose its being held by any one who is familiar with the districts in which these rocks prevail. It may safely be affirmed that there is no one indication or feature of true aqueous stratification which is awanting, except the presence of organic remains. There are the same alternations of siliceous, muddy, and calcareous beds, which everywhere characterise a long continuance of marine deposit thrown down under various mineral conditions.

It is well to observe that this new classification of the rocks in the north-west of Scotland adds additional force to an argument long ago used by Sir Roderick Murchison in reference to the bearing of geological evidence on the great question of the beginning and succession of life. The Silurian strata, in which fossils have been discovered, are more crystalline and more highly metamorphic than the Cambrian strata which lie below them. Yet, in Scotland at least, no organic remains whatever have as yet been discovered throughout the vast series of beds which belong to those old deposits; whilst elsewhere the few forms of life hitherto discovered indicate what M. Barrande has called a "Primordial Zone." These successive formations have now been traced, and more or less examined, in almost every region of the globe, and everywhere the same limited assemblage of organic remains has been established—the same total absence of any indication of terrestrial life—the same few generic types, chiefly of crustacea, cephalopoda, brachiopoda, most of which have long since ceased to be, whilst one at least has survived every subsequent revolution, and is still living in the present day. On the other hand, it will no doubt be argued by those who take an opposite view, that the circumstances attending this reclassification of the older rocks of Scotland tend more than ever to teach the necessity of caution in the interpretation of negative evidence. The abundant existence, it will be said, of organic life during the ages of the Silurian deposit is beyond question. Yet all traces of it have been obliterated absolutely throughout a vast series of beds: in others, the indications are so exceedingly obscure that their character is altogether doubtful; whilst only in one or two thin seams of limestone, and in still rarer quartzite beds, has an unequivocal record been preserved of the highly organised and abundant molluscan life of the Silurian seas.

Before passing from the Geology of Scotland, I must direct the attention of the Society to the very beautiful geological map of this city and its vicinity which has been lately published by the Department over which Sir R. Murchison presides as Director-General. The coal-basin, with its coal crops and faults, was the work of Mr Howel; the rest was surveyed by Mr Geikie; both these gentlemen being geologists of the Government Survey. The admirable care and exactness with which they have given the minutest details of a very varied and intricate district, is an excellent example of the high economical as well as scientific value we may anticipate of the geological survey of the country.

The oldest formation in this sheet is the *Lower Silurian*, of which two small patches occur along the southern edge of the map. They belong to the great Silurian tract of southern Scotland, against which the upper Old Red Sandstone and carboniferous rocks of the Lothians rest unconformably. There are at present known only two areas of *Upper Silurian* strata in Scotland, of which one occurs in the Pentland Hills, and is mapped in the present sheet. It consists of highly inclined shales and sandstone. Mr Charles Maclaren was the first to detect organic remains in these strata. About twenty-five years ago he found two orthoceratites, but in a fragmentary state. In the year 1857, when the Geological Survey extended into the district in question, Mr Geikie first made known the richly fossiliferous character of these Silurian strata, the assemblage of fossils unequivocally indicating the horizon of the Ludlow rocks of England. On the edges of the upper Silurian beds rest unconformably the upper Old Red Sandstones and conglomerates, with enormous interbedded sheets of felstone, which form the chain of the Pentland Hills.

The great Lower Carboniferous group is well shown in the area embraced by the present map. It occupies the whole of the district between the Bathgate hills and the Pentlands, and contains in that region a seam of limestone, which is the equivalent of the Burdiehouse limestone on the east side of the Pentland ridge, and also a seam of coal that appears to be quite local. The line of outcrop of these two seams, as traced on the map, will show the intricate character of the geological details. Perhaps the most remarkable feature in the Lower Carboniferous series of the Lothians is the abundance and variety of its associated contemporaneous igneous rocks. There is no well marked zone in the series which does not at some locality in this region display its sheets of greenstone, felstone, or ash.

The Carboniferous limestone of this sheet shows characteristically the Scottish type of that sub-formation. Its base consists of limestone bands, with associated shales, sandstone, and coals. Above these comes the group of coal-bearing strata, known as the "Edge coals" of Midlothian. But these are not the Coal-measures of England, seeing that above them there are bands of limestone, with true Carboniferous limestone fossils. The Millstone grit has not yet been satisfactorily determined, but its place may be represented by some of the thick sandstones of Roslin.

The Coal-measures proper, or "Flat coals" of Midlothian, occupy the centre of the Edinburgh coal-basin. They are truly the equivalents in position as well as fossils of the Coal-measures of England.

It is deserving of remark, that while, in the Lower Carboniferous strata and in the "Edge-coals" of Linlithgowshire, volcanic rocks abound, none occur in the Edinburgh coal-field, although they were abundant in that district during the earlier part of the Carboniferous period.

The system of parallel faulting of the Pentland Hills is also worthy of notice, as accounting for the small development of Lower Carboniferous strata on the east of the chain, and their great expansion to the west. The highly inclined character of these strata along the east side of the hills (some being quite on edge, hence called "Edge-coals"), arises from the downthrow of the whole coal-field against the older rocks of the chain. A detailed description of the sheet from the geologists before named is at present in the press.

The attention, not of geologists only, but of men of science in several departments, has, during this and the preceding year, been fully awakened to the importance of a discovery which is really of much older date—viz., that flint implements, the work of man, are found in beds of drift gravel associated with the bones of the last generation of the great extinct mammalia. The full significance of this fact is only now being fully recognised, and many of the conclusions which it may tend to establish are subject to much doubt, and will probably form the subject of increasing controversy. But it is only necessary to have a clear idea of the facts as they have been now ascertained, to see that one conclusion at least is placed beyond all question—viz., that great physical changes on the surface of the earth, and these, in part at least, effected by the agency of water, have taken place since the creation of man.

Whether this conclusion carries the creation of man farther back than had commonly been supposed, or whether it merely brings nearer to us than we had before conceived, the last great changes which have produced the existing surface, is the main question on which debate arises. As geology gives no certain data for computing positive, but only relative time, this question is necessarily involved in much obscurity. But there are certain limits within which, after all, the controversy is confined. It is well to observe that, according to the principle on which geological times and epochs are classified, the human epoch remains, after these discoveries, very much where it stood before. It is true that many of the large animals, with which the traces of man seem to be connected, are now extinct; but a very much larger number are still living. The Molluscan Fauna, which plays so important a part in ages of geologic time, is absolutely the same. The general aspect of animal life is the present aspect, with the exception that a certain number of species of the larger Herbivora and Carnivora have become extinct. But such extinctions, local in many instances, and total in some, have taken place in historic times, and are in visible process of accomplishment even now. Such extinctions do not constitute a new Fauna, nor, according to the received principle of classifying past times, do they mark a new geological age. The era of man, therefore, remains, geologically speaking, in the same relative place in which it stood before—the very last and latest of the world.

But the fact that human implements are found under great beds of gravel and of earth formed by water, whether of rivers or of the sea, at an elevation which in either case would imply changes of level, such as, if general, would be enough to revolutionize the whole aspect of our now habitable surface, is a fact which casts new and important light on the (geologically speaking) very recent date at which those changes have taken place.

Whether the men who formed the implements were or were not contemporary with the living quadrupeds whose bones are associated with these implements, seems to me a subordinate question. The mere fact of such association may not absolutely prove the point, because it is conceivable that the bones may have been merely re-aggregated from an older fossiliferous deposit. But I suspect that the reluctance to admit the contemporaneity of man with those animals results from the reluctance to admit man's priority to such physical changes as are supposed to separate us from a Fauna typified by the Mammoth and the Elk. If, therefore, the fact of such priority be proved from the stratigraphical position of the flint relics, wholly independent of any argument derived from organic remains, the importance of the question respecting the human age of the great mammals will be much diminished. It may be well, therefore, to keep our attention firmly fixed on what is the really important question—the nature and position of the strata in which, and under which, the flint implements have been interred. Going no farther for light upon this question than the particular beds at Amiens and Abbeville in France, where the implements have been found in greatest abundance, it is enough to record the facts. The flints are embedded in a stratum of gravel, which rests directly on an eroded surface of the chalk, and contains along with the hatchets the bones of the great extinct mammalia. This is again surmounted by a bed of sand from seven to ten feet thick, in which only a few rare bones and implements have been found. This is again capped by a second bed of gravel from two to five feet thick; and lastly, on the top of all, is a bed of brick-earth, in which, as if to afford the very poetry of illustration, are to be seen the tombs of Roman Gaul. Such is the position of the beds with reference to each other. But what is their position with reference, not to each other, but to the surrounding country? The gravel-bed extends to points upwards of a hundred feet above the level of the river Somme, which occupies the bottom of the existing valley. It is described by Professor Rogers, a most competent and accurate observer, as extending to the summits of the plateaux which determine the existing drainage. Whether, therefore, the water which formed those beds were marine or fluviatile, in either case such changes of level are implied as would be sufficient, if general, to alter widely the existing distribution of land and sea.

Here, then, the question arises, Were those changes local—confined perhaps to the district of Western France? Connected with this question, another immediately occurs: Is not this bed of gravel identical in character and composition with similar deposits in other countries? Is there anything to distinguish it from the gravels containing precisely the same mammalian bones which are familiar to geologists in almost every country, and which have been recognised every here and there over the whole of Europe, from Siberia to Palermo, and from the basin of the Thames to the valley of the Danube? So far as I have been able to gather from the papers which have detailed the facts, there is nothing to indicate any difference whatever, except that, at least until this discussion arose, human implements had nowhere else been recognised as associated with the drift. The absence of such remains elsewhere, however, would go for little in establishing a difference, because it is clear that the men who existed before the formation of the Abbeville beds were rude, and probably widely scattered savages, distant outliers of their race. The chances, therefore, were infinite against the preservation either of them or of their works. But even this distinction, it would appear, is being broken down. It is now recollected that so long as sixty years ago, human implements had been discovered in Suffolk under similar conditions, and the fact communicated to the public in an archæological journal by the discoverer Mr Frere. The spot has been since visited by Mr Prestwich, fresh from the Abbeville beds, and he recognises the same phenomena. But this is not all. The scent, once taken up, is becoming stronger and stronger, every day. Closely connected with the period of the drift-gravels are the ossiferous caves and caverns so common all over Europe where limestones prevail. They have been long known to contain a profusion of bones of the extinct as well as of living mammalia. Here, again, it is now confidently asserted that human implements are being found under conditions which leave no doubt that, whether man was or was not contemporary with these animals, he must at least have preceded the action of those agencies which brought the bones together. The evidence in this case must necessarily be more liable to erroneous interpretation than in the case of implements found in undisturbed beds of gravel, because caverns must at all times have been a resort of savage tribes whenever the entrances were accessible from the surface. But the evidence seems to be such as is sufficient to convince examiners so careful and acute as Dr Falconer and Mr Prestwich of the undoubted priority of man to that diluvial action which appears to have swept into those caverns their mixed contents. But this is not all. It is now recalled to mind, that so long ago as 1833, a M. Schmerling had published Researches into the Ossiferous Caverns of Belgium, in which, not implements of man only, but his teeth and his bones, and portions of his skull, had been found so thoroughly mixed up with the remains of the lower mammalia as to leave in his mind no doubt, if not of their contemporaneous life, at least of their contemporaneous entombment in the spots where they are now found. These are remarkable facts; and in so far as they indicate that the phenomena of Abbeville are closely related to others observed in many different parts of Europe, they go far to prove that the French gravel-beds were due to no mere local cause, but to some diluvial action which was general, and therefore in all probability due in great part to the waters of the sea.

I need not point out how many and how interesting are the questions which this discovery raises in our minds. Was this incursion of the waters of the sea, over a pre-existing land, sudden and transient, or gradual, and of long duration? In the Abbeville beds there seems to be clear evidence of four successive stages of submergence, each distinguished from the other by different mineral conditions. The first bed, that in which the bones were entombed along with the human implements, indicates an action strong, if not violent, but not of long duration. The second indicates, by its finer materials, the action of a gentler force. The third seems to be very much a repetition of the first; whilst the last can only be accounted for on the supposition that fine sediment had time to accumulate in comparatively tranquil waters. The interest of the question is very much centred in the nature of the action which began this series of events. Perhaps it may be well to look at the conclusion come to in respect to the origin of the mammaliferous drift-gravel by the geologist who has devoted most special attention to the subject, and before the discoveries of Abbeville had disturbed any preconceived idea. I find Mr Prestwich, in a lecture delivered in 1857, coming to this conclusion in respect to the ossiferous gravels of the Thames:—"Taking into consideration the absence of contemporaneous marine remains, and noting the immense mass of but slightly worn débris derived from and covering irregularly the sedimentary deposits; and the fact that it has evidently been transported from greater or less distances, combined with the occurrence in the gravel of the remains of large land-animals, of trees, and of fresh-water land-shells, we have, I conceive, at all events in these facts, indications of at least one land-surface here destroyed, and its rocks, plants, and animals involved in one common wreck and ruin."

An able and elaborate paper on the "Distribution of the Flint-Drift of the South-east of England," &c., was communicated to the Geological Society of London by Sir R. Murchison in 1851. The phenomena he describes seem everywhere to be a precise repetition of those of Abbeville. Everywhere the flint-drift, which is often, as there, covered by brick-earth, clay, or loam, is characterised by the bones of the great extinct mammalia, and everywhere, according to the author's view, gives evidence of sudden and violent diluvial action. Everywhere, also, this drift-gravel rises high above the levels of the existing drainage, whilst, at the same time, it gives evidence that the general configuration of the surface was substantially the same as now. Everywhere, also, wherever shells have been preserved, they belong to our existing fauna, and thus prove beyond a doubt that, geologically speaking, the age of the drift is the age of the existing world. "In short," he says, "the cliffs of Brighton afford distinct proofs that a period of perfect quiescence and ordinary shore action, very modern in geological parlance, but very ancient as respects history, was followed by oscillations and violent fractures of the crust, producing the tumultuous accumulations to which attention has been drawn."

Unless, then, the Abbeville beds of drift can be separated from those so widely prevalent in other countries, the discovery of human implements underneath this drift will rather tend to bring nearer to us than had ever been supposed some great and sudden diluvial action, than to cast any very clear light on the absolute time—that is, on the time measured by years or centuries—which has elapsed since the creation of our race. The facts which have been brought to light prove, indeed, clearly enough, that since man walked the earth some great changes have affected the condition of its surface; and it is impossible as yet to say what bearing this discovery may be found to have on that remembrance of at least one great catastrophe, which is not more a part of sacred history than it is of profane tradition.

We must not, however, shut our eyes to the indirect effect which this discovery must have on the question of positive time. In the first place, there is a school of geologists, led by our distinguished countryman Sir Charles Lyell, who disbelieve generally in those conclusions which point to violent and sudden changes; and, in the next place, it must be remembered that changes which in point of geological time might well be accounted rapid, might nevertheless well occupy thousands of our years. There is proof in those gravel-beds of the Somme of a double motion, one of submergence to the depth of certainly more than 100 feet, another of subsequent elevation, during which the immense mass of material which had been brought down and deposited by water, has been worn through and broken into escarpments, either by the existing stream or by more powerful currents. We have no data from which to measure in years the time which the accomplishment of such a series of changes may imply. But I think the general impression left upon the mind must be in favour of a very high antiquity. Farther light may be cast upon this subject if the drift-gravels of France, the south of England, and other countries, can be co-ordinated with any one of the stages of operation to which we owe the superficial deposits of Scotland and the north of Europe generally. It is well known that in these last there is one prominent characteristic which is absent farther south. I mean the abundant proofs of glacial conditions, or an arctic climate. On this subject there is a paper of great interest in the last "Quarterly Journal of the Geological Society," by Mr Jamieson, founded on observations made mainly in the county of Aberdeen. The cycle of changes which this geologist thinks can be clearly traced, as necessary to account for the superficial deposits of our own country, amount to no less than five great epochs, including two of submergence and two of elevation, and involving changes of level to the extent of more than 2000 feet. Scotland has long ago furnished evidence as clear as that founded on the French flint implements, that at least previous to the last of these elevations man had reached her shores, and navigated her rivers and estuaries in those rude canoes, hollowed out of trunks of oak by stone hatchets, which have been frequently found in elevated beds of silt and gravel in the valley of the Clyde. And here we strike upon evidence which has some bearing upon the question of time. Closely connected with the period preceding the last elevation of the land, we have proof that an arctic climate prevailed over a large part of the northern hemisphere, whose climate is now comparatively temperate.

But this period seems clearly to have been one of long duration—that is to say, of such duration, and lasting under such conditions of comparative rest, as to allow the development of a glacial fauna. Close to my own residence on the Clyde, each low ebb exposes numerous examples of the *Pecten Islandicus*, and of those very large *Balani*, which are now confined to arctic seas. These beds of shells, which are all of existing species, but of species which have retired from our now more genial temperature to a northern habitat, were first described by my friend Mr Smith of Jordanhill, and his observations and conclusions have since been abundantly confirmed. We have no knowledge how this period was brought to a close. But there seems to be evidence that it had come to an end, and that for a long time before the last elevation of the land, and before man had appeared in Scotland. This seems to be a legitimate deduction from the fact that the canoes in the elevated Clyde beds are formed of oak of large dimensions and of great age. Forests which afforded such timber must have flourished in a climate not much more rigorous than that which exists at present. Here again, then, the earliest footprints of our race are traced up to a point, preceding indeed some important physical changes, but clearly subsequent to the establishment of all the main conditions which now affect the distribution of animal and vegetable life.

As regards the extinction of some animals, I have spoken as if the contemporaneousness of man with them whilst yet living ought not to be absolutely assumed merely from the fact that his implements are associated with their bones. But on this point new evidence is being rapidly collected and brought together. Mons. Lartet, a distinguished French naturalist, has found what he considers to be distinct evidence of the mark of human weapons on various parts of the skeletons of the extinct mammalia of the drift. These marks have been detected on the skull of the *Megaceros Hibernicus*, or great Irish elk,—an animal which stood some ten feet high—on the bones of the *Rhinoceros tichorinus*, and on those of various species of the ox and deer, which are now either extinct or confined to the last remnants of a declining race. The marks are of various kinds—some of them peculiar—indicating a sort of sawing with some instrument not of the smoothest edge. M. Lartet has ascertained that these blows and cuttings could not be made except on fresh bones—that is to say, on bones undried and retaining their animal cartilage. Farther, he has succeeded in producing on the bones of existing animals precisely the same peculiar forms of incision by using one of the old flint implements found in the same beds of gravel, whilst he has equally found that similar marks are incapable of being produced by implements of metallic edge. His conclusion is thus stated by himself:—"If, therefore, the presence of worked flints in the diluvial banks of the Somme, long since brought to light by M. Boucher de Perthes, and more recently confirmed by the rigorous verifications of several of your learned fellow-countrymen, have established the certainty of the existence of man at the time when those erratic deposits were formed, the traces of an *intentional* operation on the bones of the rhinoceros, the aurochs, the megaceros, the cervus sommensis, &c. &c., supply equally the inductive demonstration of the contemporaneousness of those species with the human race."

The great number of flint implements which have been found in the French beds—said to amount to upwards of a thousand in a few years—when compared with their great rarity elsewhere, is not perhaps so curious as at first sight it may appear to be. Flint implements can only be made where flints are accessible; and it is well known that the flints of particular beds, or strata, of the chalk, are more easily fashioned than others. It is therefore probable that some such favourable locality had existed in the chalk of that part of France, and that what may be called a manufactory of them had consequently been established there. It is remarkable that some of the implements are only half finished, whilst all of them exhibit such sharp edges and angles as are sufficient to prove that they have not been transported far from the spot where they were made, nor subjected to long wear from use.

On the whole, then, it is not to be doubted that the discovery of human implements under repeated beds of aqueous drift and sediment, so high above the levels of existing rivers, or of the existing sea, is a fact of very great significance and importance. In its bearing on geology, it is principally interesting as proving at how recent a period portions at least of the earth have been subject to powerful and rapid diluvial action. In its bearing on human chronology, everything depends on the degree of suddenness and rapidity with which water may have been brought to act upon the former surface. But here anything like data for positive computation entirely fails us. We have no knowledge, in historic times, of any aqueous operation on so grand a scale. Making, however, every deduction which can be made, we must be prepared to find that the facts thus brought to light in the valley of the Somme will be held to furnish important collateral evidence in support of the reasoning founded on other sciences, such as philology and ethnology, which has long demanded, for the development of our race, a number of years far exceeding that which is allowed by the chronology previously received. It is the beautiful expression of Sir Thomas Browne, which I find quoted by Dr Mantell in a former paper on this subject, that "Time conferreth a dignity upon the most trifling thing that resisteth his power;" and it is impossible to look at these rude implements—perhaps the earliest efforts of our race, in the simplest arts of life—without being impressed with the high interest of the questions with which they seem to be inseparably connected.

I think it is impossible not to consider the publication of Mr Darwin's work on the "Origin of Species" as an event in the history of scientific speculation. The influence which such theories have had in stimulating and directing the progress of actual discovery, entitles them, when they come from distinguished men, and when they rest on any large amount of careful observation, to the marked attention of such Societies as this. It cannot be denied that Mr Darwin's book claims our respect on both these grounds. It may be true, as I think it is, that all the facts he has brought together, supposing them to be clearly established (or even much extended by the volume of proof which is still in reserve), bear a very small proportion to the purely speculative conclusions which go to make up his theory on the "Origin of Species." Yet probably there is no other man now living who could have made such a rich collection. No other man since the death of Humboldt has had such powers of observation, combined with such opportunities of observing. "The Voyage of the Beagle" shows how large and wide has been his experience of the general aspects of nature; whilst his monograph on the Cirripedes, and other papers on zoology, testify to his unwearied assiduity in the examination of detail. His book, therefore, comes before the world with every claim to respectful consideration which can be founded on the high scientific reputation of its author. The "Origin of Species," however, means nothing less than the method of creation; and this is a subject so profoundly dark, that no amount of existing knowledge can enable any man to do more than walk carefully round its outer margin, noting where, here and there, some fact, more significant than others, seems to give hope of entrance into the obscurity within. The particular theory advanced by Mr Darwin is but a special form of the old theory of development; special in this respect, that it professes to point out the particular law under which every animal and vegetable form may have been derived from those pre-existing, by ordinary generation. One general admission may, I think, be safely made in reference to all such theories. They are undoubtedly more easily conceived than what is called "creation." But this is not saying much. The truth is, that creation, of which we often talk so easily, is a work of which we have no knowledge and can have no conception. Something is known of the laws under which organic beings, once created, are enabled to continue their existence and to propagate their kind; and it is, of course, comparatively easy for us to conceive some such modification of those laws as Mr Darwin suggests,—to suppose that any given animal should occasionally produce offspring slightly different from itself in some one portion of its structure, and that such differences should go on accumulating, until finally they end in the most divergent forms. But to imagine processes which shall be the most easily conceivable goes but a very little way in science; and, after all, the difficulty is but postponed. Mr Darwin himself is obliged to have recourse at last to the ordinary forms of language in which the idea of creation is expressed, and speaks of a primordial form into which "life was first breathed." In science we may sometimes allow the question to be asked, "What is most easily conceivable?" but only on condition that it be followed hard by the farther question, "How much of this easiness of conception is gained at the expense of departure from the evidence of facts and the experience of nature?" In answer to this inquiry, it may well be doubted whether Mr Darwin has proved one single fact capable of sustaining the very first step in his ingenious argument. That argument seems to be as follows: Man has succeeded by "artificial selection"—that is, by careful "breeding"—in establishing certain modifications in the forms of domestic animals. Therefore, similar results may be produced to an infinitely greater degree by nature. Only, the principle of selection will be different. Man chooses those qualities which are most useful to him as master. Nature will choose those which are most useful to the animal itself. But the qualities which are most useful to an animal will be those which enable it to survive when its fellows and congeners die. If, therefore, any such qualities arise in any particular family or breed, they will be preserved and perpetuated. This is a beautiful theory. But when we ask how far the facts carry us towards the "origin of new species," we find that there is in reality no perceptible advance. The changes producible by breeding, or by "artificial selection," are all confined within a circle which indicates a restraining law. The changes producible by "natural selection" are, so far as we know and can observe, under similar, if not under still narrower limitation. As regards the first, Mr Darwin himself supplies us with an illustration beyond all others striking, of that law of reversion to type, the existence of which he nevertheless disputes. Pigeons are his favourite example of extreme modification of form. They have been "bred" or selected for three thousand years. Mr Darwin took two of the unlikest and most aberrant parents he could select, a black "barb" and a white "fantail." The result was, that a grandchild of these parents exhibited a close return to the old primal type, the rock-pigeon, from which all domestic pigeons originally sprung. Yet who knows through how many generations of "selected parents"—perhaps from the days of the Pharaohs—this chick had inherited its ancestral colours! Can there be a stronger illustration of that restraining law of reversion to type, which, so far as we know, confines within a very narrow circle, not only the extent, but the duration of aberrant forms of life? Then, as regards natural selection, do we know of any one authentic instance in which new conditions of life have been met by such modifications of structure as might enable an animal to survive its congeners in the "battle of life?" Our experience in this way is perhaps fully more extensive than in any other. The truth is, that man is himself the greatest modifier of the natural conditions under which the lower animals are placed. He is year by year producing revolutions which might be equivalent to centuries of natural change. Nor are these without a powerful effect on animal life. Mr Darwin has traced the changes thus produced with singular ingenuity and beauty of description. But all those changes are produced by the substitution of one species for another,—never by the modification of the same species to the new conditions which surround it. There need be no dispute that, under the law so beautifully traced by Darwin, such modifications, *if they did arise*, would tend to survive and be perpetuated. But what we want is—facts to justify the supposition that any such modifications do actually arise; such, for example, as would enable an animal adapted for marshy land to survive on land which had become dry;—or arboreal forms to survive the destruction of their native forests;—or land-animals to adapt themselves to a country which is being gradually submerged. These are all operations of which man has had experience, and to some of which he is every day contributing; yet no instance is recorded of nature having ever had any opportunity of exercising in favour of any animal that "selecting" power which is the assumed origin of new species. The Fauna is indeed changed by such changes of condition as I have supposed. But that change is effected by substitution, not by conversion. One animal or plant invades the former territory of another. In our own country, for example, the grouse gives way to the partridge, or the snipe to the landrail; or, more rarely, the lark may be supplanted by the waders and the gulls—the field-mouse and the mole by the water-rat and the otter. But in no case that we know of, or that Mr Darwin has adduced, has any wild animal been enabled, by any modification of form, however slight, to survive any essential changes in that condition for which it was first adapted. And as this is the law which obtains in the present, so also it is the law which appears to have obtained in the past. The absence of any evidence of the passage of one form into another, discoverable in the records of former worlds, is confessed by our author himself. All his arguments are directed, not to deny this fact, but to explain it. It has been truly said, in a very able and interesting paper on the subject which was communicated to this Society by one of its members early in the present year, that "The strongest points in favour of the general results come to by Mr Darwin, are a class of facts which can scarcely be said to bear distinctively on his theory more than upon various other theories already promulgated, and more or less adopted. One of these is the fact that all animals and plants, throughout all time and space, should be related to each other in group subordinate to group, ... another not less formidable fact is the existence of the same homological parts in different animals, sometimes aborted, and sometimes largely developed." The endeavour to explain and account for these strange connections and relationships is one of the highest aims of science. To refer them to the great law of hereditary descent is a very natural suggestion, and for a moment some minds may be disposed to rest in it as a kind of explanation. Reduction to a known and familiar law is the nearest approach to explanation which science can afford. But we must beware of the subtle error which lies in changing a law well known and familiar, into another law entirely unknown and new, by ascribing to it effects and operations of which we have no experience. If the law of descent by ordinary generation is consistent with the origin, through this means, of new species, some proof must be given of the fact. Until such proof is adduced, the assumed law is not that of *ordinary* generation, but of *extraordinary*—of a new kind of generation essentially different from that of which we have any knowledge.

It is well worthy of remark, that Mr Darwin holds strongly to the doctrine of "single centres of creation"—or as, to suit his special theory, they ought perhaps to be called, single centres of birth. He believes that each new species came into being at some one spot only, and that, however wide may be its distribution now, such distribution has been due wholly to dispersion. "If the same species," he says, "*can* be produced at two separate points, why do we not find a single mammal common to Europe, Australia, and South America? The conditions of life are nearly the same." But surely this belief in single centres of creation or of birth is not very easily reconcilable with the rest of Mr Darwin's theory. The essential idea of that theory is, that new species arise from any accidental variety which enables the animal possessing it to have some special advantage in the struggle for existence. But, as similar modifications of structure would in this respect confer similar advantages, at one time or other, under some circumstances or other all over the globe, it is impossible to understand why they should not frequently arise at many different points, either at once, or in succession. We may freely grant, therefore, to Mr Darwin that his reasoning explains to us how a given species, *once born*, and which begins the battle of life under favourable conditions, should rapidly spread, and should extinguish its congeners and predecessors, which are less favourably endowed. But it gives us no sort of explanation, or even suggestion, of the law *under which any such new species is first produced.* How such a new birth comes to be determined, and above all how it can only be determined at some one spot of all the million spots on which the same parents flourish, remains as profound a mystery as before; and we have in reality not advanced a single step towards the "origin of species."

The conclusions arrived at by Mr Darwin are essentially but another form of the old theory of development, and as such they will meet with the same vigorous resistance. We may cordially join in the warning of Professor Huxley, that the arguments of such a naturalist as Mr Darwin must be met on scientific grounds alone. And yet the difficulty, to use no stronger word, of reconciling this theory when applied to man, with all that we know of his physical and moral nature, and all that we have hitherto believed respecting his early history, is at least one among the many difficulties which may well call for the most jealous and critical analysis of every step in Mr Darwin's argument. He himself, indeed, seems to feel no difficulty in the matter—lineal descent from some early fish or reptile—"some ancient prototype furnished with a floating apparatus or swimming-bladder"—Mr Darwin regards as the noblest claim of ancestry. "When I view all beings," he says, "not as special creations, but as the lineal descendants of some few beings who lived long before the first bed of the Silurian system was deposited, they seem to me to be ennobled." I am afraid that the honour of this parentage, as regards our own species, will not be universally appreciated. The question, however, is not whether it be "ennobling" or the reverse, but whether it can be proved or rendered in any degree probable. Yet, in judging of the sufficiency of evidence, it is well to recollect the full weight of the conclusion which that evidence must be strong enough to bear; nor, in this point of view, do I think it wholly unphilosophical to bear in mind the innate beliefs and instincts of mankind.

It is not, however, my duty or my desire, in this place and on this occasion, to enter more deeply into the specific argument on the "origin of species;" I would rather indicate wherein the discussion, and the argument which has raised that discussion, has most directly tended to the advance of science. In this respect, it is not too much to say that the whole book is full of the most curious and original observation, and exhibits in an eminent degree that power and habit of arranging and co-ordinating physical phenomena which is essential to the attainment of great results, and which it has been the special use of such theories in the history of science to evoke and to direct. In particular, I think no one can read Darwin's chapter on the "struggle for existence," or the two chapters on "geographical distribution," without feeling that new and important light has been cast on subjects which are as interesting as they are difficult and obscure.

SCIENTIFIC INTELLIGENCE.

BOTANY.

Plants yielding Tea.—I agree with Choisy, that there is only one species of the plant yielding the tea of commerce, for which I adopt the name of *Thea Chinensis*, given to it by Linnæus in the first edition of his "Species Plantarum," and afterwards sanctioned by Sims and others as a collective one for *Thea Bohea*, *viridis*, and *Assamica*. The propriety of taking the name "*chinensis*" may be open to discussion, since we have no wild specimens of tea from China, but only from Upper Assam; and a Chinese tradition of great antiquity states that the Tea-plant was introduced from India by the Buddhist priests, so that it is possible we may term a species "Chinese" that is in reality "East Indian" in its origin, and thus further another instance of "*lucus a non lucendo.*" Dr Hooker, to whom I communicated these doubts, thinks that the plant may yet be found wild in North-Western China; and his intimate knowledge of the Flora of India renders that opinion of the utmost value.—*Seemann in Linnean Society's Transactions*, vol. xxii. p. 350.

On the Coal-Plants of Bohemia. By M. Stur.—The fossil plants from the Coal-basin of Rakonitz (Bohemia) present fifty-three species, and may be subdivided into four distinct local floræ. These species are distributed among twenty-one genera of ten families.

Calamites,	3
Asterophyllum, Annularia, Sphenophyllum,	6
Neuropteris, Nœggerathia, Schizopteris, Dictyopteris,	6
Sphenopteris,	5
Asplenites, Pecopteris, Alethopteris, Cyatheites,	13
Stigmaria,	1
Sigillaria,	4
Lepidodendron, Knorria, Lepidostrobus, Cardiocarpon,	12
Cordaites,	2
Flabellaria,	1

The following species—*Calamites communis, Annularia fertilis, Nœggerathia foliosa* (particularly frequent around Rakonitz), *Alethopteris pteroides, Cyatheites Miltoni, Stigmaria ficoides, Lepidodendron Haidingeri, and L. aculeatum*—as also the *Sigillarieæ*, occur generally in considerable number as individuals.

Calamites communis, Annularia fertilis, Cyatheites oreopterides, C. Miltoni, C. arborescens, Stigmaria ficoides, Cordaites borassifolius, and *Flabellaria Sternbergi*, occur both in the shales forming the roof of the coal-bed and in those immediately beneath it, so that the flora of both these shales may be considered as nearly identical; and the existence of *two distinct floræ* within the basin of Rakonitz may indubitably be admitted: the one of *Asterophylliteæ, Neuropterideæ, Sphenopterideæ,* and other genera of *Filices* and *Lycopodiaceæ*, containing but a small portion of carbonaceous substances; the other, *Sigillariæ* and *Lepidodendra*, which have chiefly contributed materials to the coal-bed. Two species (*Knorria imbricata* and *Lepidodendron* (*Sagenaria*) *Veltheimianum*), found in some few fragments within the Rakonitz basin, are characteristic of the lowest strata of the Carboniferous group.

The flora of this basin bears a striking general resemblance to that of Radnitz (known long ago through Count C. Sternberg's splendid publications), and to the flora of the coal of Zwickau (Saxony), lately described and figured by Professor Geinitz. Some few species, however, occurring in the basin of Rakonitz, are wanting in the coal-measures of Radnitz; these are *Sphenopteris rutæfolia, Asplenites cristatus, Alethopteris aquilina, Al. pteroides, Al. muricata, Cyatheites Miltoni, Cy. unitus, Cy. dentatus, Sigillaria mammillaris, S. oculata, S. elongata.*—[*Proceed. Imp. Geol. Instit. Vienna, March* 13, 1860.—*Translated in Quart. Journ. of Geol. for Nov.*]

Flora of Amoor.—Asa Gray, in his notice of the *Primitiæ Floræ Amurensis*, by C. J. Maximowiez, says—The Amoor flora offers several species identical with peculiarly Eastern North American ones: e.g. *Acer spicatum, Pilea pumila, Asplenium thelypteroides, Symplocarpus fœtidus!* Also several closely representative ones; such as *Caulophyllum robustum*, doubtless the same as the Japan plant, which in fruit answers perfectly to our *C. thalictroides*, and I still suspect not distinct from it; and *Maximoviezia chinensis*, Rupr. (to which evidently belongs *Sphærostema japonicum*, Gray), a close counterpart of our *Schizandra; Acer tegmentosum*, very nearly our *A. pennsylvanicum; Hylomecon vernalis* which seems very close to our *Stylopnorum diphyllum;* and *Plagiorhegma dubium*, which has the look of a monstrous dicarpellary *Jeffersonia*. Indeed, good flowers are still wanting to make it at all certain it is not a *Jeffersonia!*

Very remarkable indeed is this division of monotypic or nearly monotypic genera or groups between North-eastern Asia and North-eastern America,—of which so extended a list can now be given,—and very suggestive is it (at least where the species are identical or nearly so) of a comparatively recent communication between the two countries.—*Silliman's Journal, May* 1860.

GEOLOGY.

On the Structure of the North-west Highlands, and the relations of the Gneiss, Red Sandstone, and Quartzite of Sutherland and Ross-shire. By Professor JAMES NICOL, F.G.S.—The author first referred to his paper in the "Quarterly Journal of the Geological Society," vol. xiii. pp. 17, &c., in which the order of the red sandstone on gneiss, and of quartzite and limestone on the sandstone, was established, and in which the relation of the eastern gneiss or mica-schist to the quartzite was stated to be somewhat obscure, on account of the presence of intrusive rocks and other marks of disturbance. Having examined the country four times, with the view of settling some of the doubtful points in the sections, the author now offered the matured result of his observations. He agrees with Sir R. Murchison as far as the succession of the western gneiss, red sandstone, quartzites (quartzite and fucoid-bed), and limestone is concerned, but differs from him in maintaining that there is no upper series of quartzite and limestone, and that there is no evidence of an "upward conformable succession" from the quartzite and limestone into the eastern mica-slate or gneiss—the so-called "upper gneiss." The "upper quartzite" and "upper limestone" the author believes to be portions of the quartzite of the country, in some cases separated by anticlinals and faults, and cropping out in the higher ground, and in other instances inverted beds, with the gneiss brought up by a contiguous fault and overhanging them. This latter condition of the strata, as well as other cases where the eastern gneiss is brought up against the quartzite series, have, according to the author, given rise to the supposed "upward conformable succession" above referred to. In some cases where "gneiss" is said to have been observed overlying the quartzite, Professor Nicol has determined that the overlying rock is granulite or other eruptive rock, not gneiss.

The sections described by the author in support of his views of the eastern gneiss not overlying the quartzite and limestone, but being the same as the gneiss of the west coast, and brought up by a powerful fault along a nearly north and south line passing from Whiten Head (Loch Erriboll) to Loch Carron and the Sound of Sleat, are chiefly those which had been brought forward as affording the proofs on which the opposite hypothesis is founded; and in all, the author finds eruptions of igneous rocks, and other indications of faults and disturbance, depriving them, in his opinion, of all weight as evidence of a regular order of "upward conformable succession." Professor Nicol describes, first, the Durine section from Far-out Head to Loch Erriboll, and insists that the gneiss of Far-out Head does not overlie the limestone, but that the latter is the highest formation here, and that the country is much disturbed by north and south faults. 2. At Camas-an-Duin, intrusive granulite, where it rises up beneath the quartzite, involves large pieces of mica-slate, showing that the latter is the lowest rock. 3. At Arnabol Hill, on the continuation of the fault, the quartzite dips apparently below the igneous mass of the hill; but the openings of the Annelid tubes and the ripple marks, belonging to the surface of the beds, are here on the lower faces, showing reversal. 4. At Drium-an-teuigh it is not gneiss, but granulite that overlies the limestone and quartzite. 5. At Whiten Head, felspar porphyry intrudes in the line of junction, partly on the quartzite, but chiefly on the old slates to the east. The author refers the disappearance of the fucoid-bed and limestone (the upper part of the series), and the presence only of the quartzite in contact with the eastern gneiss, to denudation on a line of fault. He states also that there are clear sections in this district, north of Heilim Ferry, to show that the so-called "upper quartzite" passes regularly under the limestone. 6. Near the head of Loch Erriboll, the igneous matter has generally thinned out, letting the quartzite come close against the mica-schist; but when present, whether in mass or in veins, it affects the mica-slate far more than the quartzite, thus proving the schist to be the lower rock. At places the sections are much complicated by the igneous rock. Above Erriboll House, the section of the hill side, passing upwards from limestone to fucoid-beds, quartzite and mica-schist, with red felspar veins, is regarded by the author as clearly indicative of an inversion on the line of fault; this he saw also on the Ault-na-harra Road. 7. East of the Kyle of Tongue are some patches of conglomerate lying on the eastern gneiss. These have hitherto been regarded as old red sandstone; but Professor Nicol, on examining them, found that they are identical with the conglomerate of the red sandstone ("Cambrian" of Murchison) of the west coast. At Cnoc Craggie, quartzite lies on this eastern conglomerate. These remnants are supposed to have been preserved from denudation on account of having been hardened by the syenitic eruption of Ben Laoghal. 8. At the north-west end of Loch More the so-called "gneiss," overlying the quartzite, is eruptive granulite. The quartzite is thin, and has probably therefore been denuded along the line of fault. 9. At Lochs Glen Coul and Glen Dhu the gneiss does not overlie the quartzite: from a distance it may appear to do so, but the ridges are separate, and the so-called "overlying gneiss" is sometimes intrusive syenite, sometimes vertical masses of granitic gneiss with unconformable strike. 10. The quartzite of Loch-na-Ganvich dips against the syenite of Glasven, which mountain is not all quartzite, as has been stated, but has syenite with vertical granitic gneiss; the latter underlies quartzite, and is continuous with that of Central Sutherlandshire. 11. The syenite of Glasven does not bring up any limestone under the quartzite, and therefore the latter is the lowest of the series here; nor does the limestone of Stronchrubie dip below the quartzite. Professor Nicol traces a synclinal of quartzite and limestone from Queenaig to Brebag, resting at each end on red sandstone and gneiss; and he makes the quartzite of Glasven and Ben More to be the ordinary quartzite lifted up and denuded of the limestone, not an "upper quartzite." The author states also that some of the so-called "upper quartzite" here is granite, and some of it red sandstone ("Cambrian"); and that the "upper quartzite" and "upper limestone" of Loch Ailsh and Strath Oykil are merely the repetition (on the other side of the anticlinal of Ben More) of the limestone of Stronchrubie and Assynt, modified by denudation along the line of fault. 12. On Loch Borrolan, red porphyry alters the beds. At Cnoc Chaorinie the limestone series is absent, and the mica-slate is brought against the fucoid-bed. Near Loch Ailsh the limestone is in force, but nowhere passes under the gneiss, though the latter is seen for a thickness of some hundred feet. 13. To the south of the foregoing localities the anticlinal of Ben More has been swept away, the line of junction is in the continuation of the synclinal of the Gillaroo Loch, and the gneiss is almost continuous from west to east. 14. The section at Craig-na-Cnockan proves the existence of the fault with powerful lateral compression. 15. Loch Broom gives the author similar evidence. 16. At Loch Maree and Gairloch no section shows an "upward conformable succession." The limestone is here diminished by denudation along the fault. 17. The mountains east of Loch Torridon show red sandstone and quartzite powerfully faulted and crushed, and the eastern gneiss meets them suddenly end to end—well illustrating the true structure of the district. 18. Near Loch Carron the section clearly shows the great fault. 19. At Loch Keeshorn the author sees great disturbance of the strata and no "upward succession." 20. In the south of Skye the red sandstone ("Cambrian") rests unconformably on the gneiss, and this is identical with that of the mainland ("eastern gneiss.")

Professor Nicol further argues that the mode of the distribution of the rocks shows that there is through Sutherland and Ross-shire a real fault, and no overlap of eastern gneiss of more than a few feet or yards at most; and that the fact of different strata of the quartzite series being brought against the gneiss at different places supports this view, and points to a great denudation having taken place along the line of fault. Though the quartzite is here and there altered by the igneous rocks, yet it is truly a sedimentary rock, and so is the limestone; but the eastern gneiss or mica-schist is a crystalline rock throughout: this fact, according to the author, is inimical to the hypothesis of the eastern gneiss overlying the limestone and quartzite. It has been insisted upon that the strike of the western gneiss is different from that of the east; but the author remarks that the strike is not persistent in either area, and that great movements subsequent to the deposition of the quartzite series have irregularly affected the whole region. With regard to mineralogical characters, Professor Nicol insists that both the eastern and the western gneiss are essentially the same. Both are locally modified with granitic and hornblendic matter near igneous foci; but no proof of a difference of age in the two can be obtained therefrom. The alteration in bulk of the gneiss in the western area, by the intrusion of the vast quantities of granite now observable in it, may perhaps have caused the great amount of crumbling and faulting along the north and south line of fault, dividing the western from the eastern gneiss—a fault comparable with and parallel to that running from the Moray Firth to the Linnhe Loch, and to the one passing along the south side of the Grampians.—*Proceedings of Geological Society, London, Dec.* 6, 1860.

ZOOLOGY.

Mammalia and Birds of Arctic Regions.—We have received from Sir John Richardson the following extracts from the correspondence of Mr Bernard R. Ross of the Hudson's Bay Company; and, as local lists of animals are of much utility in giving correct information of their distribution, especially when drawn up by so careful and judicious a naturalist as Mr Ross, lists of the animals and birds collected are added. These animals and birds were collected on the 62d parallel, or still more to the northward, up to parallel 67½, near which the most northerly post (that on Peel River) is placed:—

20th June 1860.—"I received charge of this district in 1858. It is greatly altered since you saw it. There are now missionaries of all sects moving about, and a more civilised tone pervades our society. I am endeavouring to improve the place as much as possible, and will, when it is complete, have a very nice house, with a real library containing above 1000 volumes. I have this winter written a longish treatise on the fur animals of this district, all of which, as far as the bears, I have redescribed; and in doing so I have examined an immense number of skins. The most interesting fact is the existence of a species of marten on the N.W. Arctic coast, resembling more nearly the *M. zibellina* of Asia than the *M. americana.* The affinity of the blue and white fox has also been fully examined into. It is perfectly certain that the colour is not dependent on the age of the animal, but I incline to the opinion that they are varieties of the same species. In all about 800 zoological specimens have been sent out by the officers of this district, of which about 600 were contributed by myself. I have, I think, identified all the species correctly—at least as correctly as I could by the aid of books alone.

" In the list there is a *Bernicla Barnstonii*, which I fancy is new. It is fully as large as a Canada goose, has sixteen feathers in the tail, two large white spots over the eyes, and the whole forehead is sprinkled with smaller spottings. A white point runs into the lower mandible, which is wholly black in all the other species. The colour of the body is darker than in the Canada goose, and the belly is of a *bright* buff colour. The *Plectrophanes picti* were very numerous this spring, and I secured about thirty. The *Pl. nivalis* and *Pl. lapponicus* were also common, and *Pl. Macownii* rarer.

" An American naturalist is here at present, sent by the Smithsonian Institution."

List of Mammalia, 31 *in number.*

Sorex Richardsonii
Lynx canadensis
Canis familiaris, var. lagopus
Vulpes fulvus
  „ decussatus
  „ argentatus
  „ lagopus
  „ borealis (blue)
Mustela americana
Putorius Richardsonii
  „ vison
Gulo luscus
Ursus arctos
  „ americanus
Sciurus hudsonius
Tamias quadrivittatus
Arctomys monax
  „ pruinosus
Castor canadensis
Hesperomys cognatus
  „ myoides
  „ sononensis
Arvicola Gapperi
  „ riparia
  „ xanthognatha
Fiber zibethicus
Erethison epixanthus
Lepus americanus
Rangifer grœnlandicus
Aplocerus montanus
Ovibos moschatus

List of Birds, 94 *in number.*

Falco sparverius
Bubo virginianus
Nyctale Richardsonii
Surnia ulula
Picus villosus
  „ pubescens
Picoides hirsutus
Picoides dorsalis
Sphyrapicus varius
  „ thyroideus
Colaptes auratus
Empidonax Trailii
  „ pusillus
Turdus Pallassi

Turdus Swainsonii
,, migratorius
Anthus ludovicianus
Neocorys Spragueii
Mniotilta varia
Helmintophaga peregrina
Dendroica striata
,, æstiva
,, maculosa
Setophoga ruticilla
Hirundo lunifrons
Cotyle riparia
Ampelis garrula
Collurio borealis
Vireo olivaceus
Parus atricapillus
,, occidentalis
Pinicola canadensis
Chrysomitris tristis
Curvirostra americana
Aegiothus linaria
,, canescens
Plectrophanes nivalis
,, lapponicus
,, pictus
,, Macownii
Poocetes gramineus
Zonotrochia leucophrys
,, Gambelii
,, coronata
,, albicollis
Junco hyemalis
Spizella monticola
,, socialis
Melospiza Lincolnii
Passarella iliaca
Agelaius phœniceus
,, tricolor
Scolecophagus ferrugineus
Corvus carnivorus
Perisoreus canadensis
Tetrao obscurus, *Say*
,, canadensis
Bonasa umbella
,, Sabinii
Lagopus rupestris
Charadrius virginicus
Ægialitis semipalmatus
Strepsilas interpres
Tringa Wilsonii
,, Bonapartii
Micropalama himantopus
Gambetta melanoleuca
,, flavipes
Rhyacophilus solitarius
Tringoides macularius
Tringites rufescens
Fulica americana
Anser hyperboreus
Bernicla canadensis
,, Hutchinsii
,, Barnstonii
Anas boschas
Dafila acuta
Nettion carolinensis
Mareca americana
Fulix affinis
Bucephala americana
,, albeola
Histrionicus torquatus
Pelionetta perspicillata
Mergus serrator
Larus glaucescens
,, Andersonii
Chroicocephalus phadelphia
Rissa septentrionalis
Xema Sabini
Colymbus torquatus
,, arcticus
Podiceps cornutus

Sir John Richardson has also received a letter from Mr Spencer Baird, secretary of the Smithsonian Institution, from which the following is an extract:—

"We have been making great efforts to complete our North American collections from the Arctic Regions, by sending our taxidermist to James' Bay, one collector to Regalette, and Mr Kennicott to the Mackenzie River. The latter gentleman spent last winter at Fort Simpson, and the past spring on Great Slave Lake at Fort Resolution. He expected to go to the Yukon in the summer, and is probably there now. As one of the most industrious collectors of our country, I have no doubt that he will thoroughly explore his present field. From him and our other northern collectors and correspondents, we have already received many interesting specimens, though the best are still on the way. We have already had *Neotoma cinerea* from Fort Churchill, with other rare mammals. Considerable numbers of the large dusky grouse, with entirely black square tail (*Tetrao Richardsonii*) have reached us from the northern part of the Rocky Mountains, also *Tetrao Franklinii*, *Tetrao leucurus*, &c. Your *Larus brachyrhynchus* is a well-recognised species, and is the same with *Larus Suckleyi*, ascertained by comparison of one of your original specimens with types of the latter."

Salmonidæ of Frazer River, British Columbia.—The Indians seem to understand the migrations of the salmon. They are deeply interested in the matter, for their only food is fish, and they stink of it like cormorants in a state of decomposition. They will tell you, in so many moons "haiyoo pich," abundance of fish; or in so many moons, "hayto pich," no fish. There are certainly two kinds of salmon which resort to Frazer's River, some say three. I do not know whether they resort to the river at the same time or at different seasons. Some assert that the two kinds visit the river alternately, one kind one year and the other the next. One is called the humpbacked salmon—it is short, thick, with a pig back and full shoulders; the other is the common salmon, the same as our own. My opinion is, that both kinds ascend the river every year, but every alternate year the hunchbacked or the common salmon is in greater abundance. Their flesh is similar—an epicure could not tell which was which by the flavour. Thirty-pound salmon are very common. They ascend the river in millions, and swarm into all the tributaries of the Frazer, even 500 miles into the interior, in shoals so thickly that it would be difficult to tread on the ground in wading a shallow. Where they are scouring, they actually crush each other out of the water. As John Mahony would say, a blind man could gaff them for a month without missing a stroke. The idea amongst the Indians is, that once they go up the river they never again return; that the farther they move from the sea the more out of season they become, till they spawn, and then they sicken and die. I cannot learn that the spent fish were ever found in the lower part of the river after descending, and it is well known that after the spawning season the spent salmon are to be found dead in shoals on the banks, and in every pool and eddy, so as to taint the air. Both bait and fly have been tried, but I never yet heard of one being taken by either. Both river and sea trout are plentiful in the Frazer and its tributaries. It is curious that at all seasons of the year salmon in any quantity, good and in full season, may be taken in the Gulf of Georgia; so that it would appear that every salmon does not enter the fresh water and breed every year. I have not seen any "gravelling."—*Charters Brew, Esq., Westminster, British Columbia.*

Remarks on Frazer River Fisheries.—In March, a species of fish called the sucker is most numerous; they are good eating, average 2 lb. weight each, but are bony. In April, large salmon are first seen to come up the river, averaging perhaps 25 lb. each. In May the smelt abounds, and is larger than the fish of the same name at home, averaging say 5 to 1 lb. June and part of July very large trout are numerous. From August to October there is a run of a smaller description of salmon, averaging 8 to 10 lb.; after this, for six weeks or two months, a description of salmon called the "hooked-bill" is taken. This is not a pretty fish, and has a tendency to a red colour. Sturgeon to be had all the year. The Hudson's Bay Company purchase large quantities of salmon for exportation to the Sandwich Islands, for which they pay two cents per lb. I have heard as much as 3000 barrels of 200 lb. each mentioned as the quantity in a season, and that they could have far more if they wished. It is expected that a large business will be done in the exportation of salmon. Persons are beginning to understand the value (which is great) of the fisheries of British Columbia. It is a most extraordinary, but I believe undoubted fact, that every second year the salmon coming up the Frazer River are deformed by a sort of large hump, or, as I have heard one who earned a livelihood by fishing call it, a "horn back." I have seen very many so deformed. It is also asserted by experienced men, that there is a greater rush than usual of salmon up the rivers on every fourth year. This is expected to take place next season. Last season two men, one of whom is now employed on the North American Boundary Commission, took two tons of salmon in one night, and of that quantity 104 fish were taken at one drift.—*Geo. Stewart Mooney, Royal Engineers.*

Remarks on the Salmon Fisheries at Vancouver's Island.—The rivers possess shallows and pools alternately. The bottoms are sandy, and afford splendid spawning-beds. The climate, I should say, is much the same in temperature as that of the same latitudes in France and Great Britain; a great deal of rain on the plains and snow on the mountains during the winter months. Salmon abound in the rivers, and are captured in great numbers, both in the estuary and fresh waters. Those caught near the heads of the waters are principally speared by Indians, for whom they make the staple article of diet, while large numbers have been killed by white men with drift-nets nearer the mouths. Puget Sound swarms with fish. The drift is, I believe, preferred to the bag-net. There is no protection for the salmon by law. There is no particular ownership. The State exercises no control. All of the salmon and trout species abound. Sturgeon are numerous and large in Frazer River, reaching 13 feet in length. Halibut are plentiful in Puget Sound. On the coast, and particularly for some distance up the Frazer River, the smelt is to be caught in enormous quantities. This, however, is only for a short season. They are so numerous at this particular time that two Indians can fill a canoe in a couple of hours, using a pole flattened at one end, and having teeth driven in its edges. This is swept through the shoal, the teeth catch in the fish, which are then adroitly turned into the canoe.—*G. S. Mooney, Royal Engineers.*

MISCELLANEOUS.

Manufacture of Iron by the Natives of Africa.—At one of the meetings of the Boston Society of Natural History, last March, Dr Hayes submitted a letter from the interior of Liberia, Africa, in which the writer says that there is no occurrence there of native iron, as stated by him in vols. v. p. 250, and vi. p. 279 of the Proceedings.

The piece there described was smelted by the tribe among whom the writer resided, who keep the art to themselves, as they find the manufacture of iron very profitable, the product of their furnaces being held in high esteem by the neighbouring tribes, as a tougher and more flexible iron than they can obtain from foreign countries. The furnaces consist of stacks of about six by five feet, and about seven feet high, with a flue in the centre about two feet long by nine or ten inches wide; the flue, passing to the bottom of the stack, is filled with layers of coal and ore, upon which they force a strong current of air by rude contrivances; nothing is allowed to escape but the dross, and a heavier brittle substance which they remelt in small furnaces; the iron is left to cool in the furnace, which gives it the appearance of ore, with large particles of dross adhering to it, much blistered in places, with very rough protuberances over much of its surface. Many of these furnaces, with their banks of dross, may be seen in the interior of the country. The colour of the ore, mostly of mountain character, is between cherry and brownish red, like red ironstone. He had seen pieces of fifty or sixty pounds, the result of one blast. They cut the mass, when heated, with their rude axes as they do wood, showing the good quality of the article. The ore is plentiful in most parts of the country, and of varying quality. The masses of iron are in many places sold as they come from the furnace; but in the interior it is forged into pieces resembling a "pudding-stick," which are used as a medium of exchange in commercial transactions in the markets and in private barter. Africa has doubtless all the iron required for her extensive wants, and dense forests for the manufacture of the coal to work it. Dr Hayes wished, therefore, to correct the error in the statement that native iron exists in Africa, to which he had been led by its texture and chemical composition, which were unlike those of manufactured iron in containing quartz crystals and magnetic oxide of iron, with no traces of carbon or its compounds.—*Mining Magazine.*

Acton Copper Mine.—This interesting locality of copper is now attracting great attention, and is yielding large quantities of ore. It is about two hours' ride from Montreal by the Grand Trunk Railroad, and about half a mile from the Acton Station.

The ore occurs in a bed of limestone, and in strata of shale bordering it. These strata and the included ores trend nearly north-east and south-west, and dip at an angle of forty-five degrees to sixty degrees to the north-west.

Attention was called to this locality about ten years ago by Sir William Logan, the provincial geologist, but no explorations were made until last year, when the soil and drift were removed. The indications of copper were much greater than before, and an abundance of copper ore was found. An open cut was commenced, and up to this time about eighty thousand dollars worth of twenty per cent. ore has been taken out. Forty thousand dollars worth of this has been sold, and, according to the lessee, about the same quantity of ore remains on the surface, not yet dressed for market. At present the average shipments of ore are two hundred and fifty barrels of the twenty per cent., or about fifty tons, worth eighty-five dollars a ton.

The ore is chiefly the variegated copper pyrites, or purple copper, and occurs in seams or irregular patches ramifying through the limestone, or disseminated in fine grains or strings through the mass, or in a cherty rock which penetrates the limestone. At the main opening the ore is found distributed in this manner through the rock over a breadth of from fifty to a hundred feet, and it is also found in the shales on one or both sides, in the condition of the yellow sulphuret. The mine is worked under a lease, and looks like a great stone quarry. It can be worked in this way for a long time; but two or more shafts have been commenced, one in the shales and one in the limestone. Labour is abundant and cheap. Good miners can be had at forty dollars a month, and ordinary labourers for eighty cents. a day. The ore is packed in flour barrels, and sent to Boston *via* the Grand Trunk Railroad, and Portland. No preparations have yet been made for dressing the poorer portions of the ore, of which a large amount is accumulating.—*Mining Magazine.*

Stephanite—Brittle Silver Ore—from Washoe.—In commercial samples of the silver ores from the Washoe mines sent to New York, I have found fine specimens of brittle silver ore, or Stephanite, in imperfect crystals, weighing from one to three ounces. Before the blowpipe it gives a large globule of silver, and decided reactions for antimony, arsenic, and copper. This mineral contains about sixty-eight per cent. of silver, and is probably the principal form in which the silver occurs with the other minerals of the vein. In most of the massive specimens of the ore where galena is the principal mineral visible, small filaments of virgin silver may be seen in the cavities, or ramifying through the mass. This silver probably proceeds, in part at least, from a change in the Stephanite.

This species is found, with other ores of silver, in the principal silver mines of Europe, at Zacatecas in Mexico, and in Peru. This is the first time it has been observed within the limits of the United States.—*W. P. Blake.*—*Mining Magazine.*

Origin of the Albert Coal of New Brunswick.—At one of the recent meetings of the Academy of Natural Sciences of Philadelphia, Professor W. B. Rogers communicated the result of observations which he had made within the last year on the structural and geological relations of the Albertite, or so called Albert Coal of New Brunswick.

An examination of the mine afforded, as he thought, convincing proof that this remarkable accumulation of asphaltic material could not have formed a part of the regular carbonaceous deposits of the region—that it is not, and never has been, a true bed or stratum, but that it should rather be regarded as a mass collected within an irregular fissure of subsequent formation, by the distillation or infiltration of asphaltic matter from the surrounding bituminous shales.

The principal features of the deposit pointing to such an origin are,—the very limited extent of the mass longitudinally traced—its sudden and great irregularities of thickness and trend—and the yet more striking fact of its transverse direction in many parts of its course, as compared with the bedding of the adjacent rocks. In the lower level, at a depth of about four hundred and sixty feet, where the combustible material has been removed almost entirely from end to end, the slaty rocks are seen in many places abutting against the sides of the mine at a steep angle, presenting frequently a jagged surface, such as would result from a transverse fracture and a gaping of the strata. The Albertite was seen adhering to these irregular surfaces, as well in the cavities as on the projections, affording, even in hand specimens, excellent examples of the discordance of the mass as to position with the stratification of the contiguous rocks.

It is worthy of note that the material thus adhering to the walls of the mine has none of that intermixture with earthy sediment which so often marks the contact of regular coal seams with the enclosing strata, but maintains the same remarkable purity as in the midst of the mass. It is, moreover, quite free from the carbonaceous and rocky debris, and other marks of mechanical violence which it must have presented had it originated in the dislocation and displacement of a coal seam originally conformable with the stratification of the neighbourhood.

These evidences of the nature and origin of the deposit are confirmed by the statement that, in the progress of the mining, several large fragments of the vertical wall-rock have been found detached and embedded in the midst of the Albertite, and on one occasion a mass of unusually great dimensions could be traced by correspondence of form to a cavity in the wall at some distance above, from which it would seem to have fallen, while the contents of the fissure were still but imperfectly solidified.

The conclusions of Professor Rogers as to the origin and nature of this remarkable deposit are thus completely in harmony with those which Professor Leidy has maintained on the ground of a microscopic examination of the material.—*Mining Magazine.*

Petroleum Wells, or Oil Springs of Western Pennsylvania.—According to letters received by Professor R. E. Rogers, it appears that some of the Petroleum wells of Western Pennsylvania had already begun to show a diminished yield of oil. This fact is in confirmation of an apprehension which he had expressed at a former meeting of the Academy, that when Artesian borings became more numerous in the favourite localities, there was a probability of such a result. Professor Rogers regards the circumstance of even a small reduction in the supply of the oil from any of the wells at this early stage of the enterprise in that region as very significant, and suggestive of the fear that, remunerative as these wells may at present prove to be, it may not be prudent to base permanent calculations upon them.—*Proceedings Penn. Acad. of National Sciences, May* 1860.

Diamond Deposits of India.—In a notice of a recently issued volume of the "Memoirs of the Geological Survey of India," the *Athenæum* gives the following abstract of descriptions of the mode of occurrence of diamonds:—

The great majority of the diamond diggings are diluvial. Against the sides of the outlaying ledges there are deep deposits of "kunkurry" and chloritic clay, in which great pits are dug in order to get at the layer of coarse subangular cherty gravel, in which diamonds are to be found. The most interesting of the diluvial mines are those of Udesma and Sakeriya. The former are in active work; but water often finds its way into the pits. Here, the best material is a stiff unctuous clay with quartz gravel dispersed through it. There are other diamond workings in the gorge of the Boghin, which must be alluvial, as the entire excavation is to be attributed to the action of a river. The natives remove some twelve feet of dark brown clayey sand to get at the boulder bed, in the base of which diamonds are found; but both here and below the narrow gorge the gravel at the surface of the river bed is much worked. Hereabouts, some twenty years ago, a European made an attempt at mining on a large scale, but with what success is yet to be discovered, although the remains of his wash-pits and picking-floors still testify to his enterprise. The author says little to encourage future undertakings of this kind, and, in fact, it is difficult to say how far the sagacity of the natives may have rightly determined the precise limits of the diamond rock. Within certain areas the ground is almost exhausted, and the natives never attempt rock diggings beyond these areas, probably for sufficient reasons. Yet it seems warrantable to infer that not a few gems "of purest ray serene" are distributed over this pebbly conglomerate, and mingled with the diluvium and alluvium of the neighbourhood. The limits of the rock-deposit should be traced, as in this part of India the rock diggings for diamonds are the most valued.

OBITUARY.

Obituary of Mr Espy.—At a recent meeting of the Regents of the Smithsonian Institution, Washington, U.S.A., Professor Bache made the following remarks:—

James P. Espy, one of the most original and successful meteorologists of the present time, died in Cincinnati, Ohio, on the 24th of January 1860, in the seventy-fifth year of his age, after an illness of a week, at the residence of his nephew, John Westcott.

The early career of Mr Espy as an instructor was marked by the qualities which led to his later distinction in science. He was one of the best classical and mathematical instructors in Philadelphia, which at that day numbered Dr Wylie, Mr Sanderson, and Mr Crawford among its teachers.

Impressed by the researches and writings of Dalton and of Daniell on meteorology, Mr Espy began to observe the phenomena, and then to experiment on the facts which form the groundwork of the science. As he observed, experimented, and studied, his enthusiasm grew, and his desire to devote himself exclusively to the increase and diffusion of the science finally became so strong that he determined to give up his school, and to rely for the means of prosecuting his researches upon his slender savings and the success of his lectures, probably the most original which have ever been delivered on this subject. His first course was delivered before the Franklin Institute of Pennsylvania, of which he had long been an active member, and where he met kindred spirits, ready to discuss the principles or the applications of science, and prepared to extend their views over the whole horizon of physical and mechanical research. As chairman of the Committee on Meteorology, Mr Espy had a large share in the organisation of the complete system of meteorological observations carried on by the Institute under the auspices and within the limits of the State of Pennsylvania.

Mr Espy's theory of storms was developed in successive memoirs in the Journal of the Franklin Institute, containing discussions on the changes of temperature, pressure and moisture of the air, and on the direction and force of the wind and other phenomena attending remarkable storms in the United States and on the ocean adjacent to the Atlantic and Gulf Coast. Assuming great simplicity, as it was developed and founded on the established laws of physics and upon ingenious and well-directed original experiments, this theory drew general attention to itself, especially in the United States. A memoir submitted anonymously to the American Philosophical Society of Philadelphia gained for Mr Espy the award of the Magellanic premium in the year 1836, after a discussion remarkable for ingenuity and closeness in its progress, and for the almost unanimity of its result.

Mr Espy was eminently social in his mental habits, full of *bonhomie* and of enthusiasm, easily kindling into a glow by social mental action. In the meetings and free discussions in a club formed for promoting research, and especially for scrutinizing the labours of its members—and of which Sears C. Walker, Professor Henry, Henry D. Rogers, and myself were members—Mr Espy found the mental stimulus that he needed, and the criticism which he courted, the best aids and checks on his observations, speculations, and experiments. But there was one person who had more influence upon him than all others besides, stimulating him to progress, and urging him forward in each step with a zeal which never flagged—this was his wife. Having no children to occupy her care, and being of high mental endowment and of enthusiastic temperament, she found a never-failing source of interest and gratification in watching the development of Mr Espy's scientific ideas, the progress of his experiments, and the results of his reading and studies; the collection and collation of observations of natural phenomena in the poetical region of the storm, the tornado, and of the aurora. Mrs Espy's mind was essentially literary, and she could not aid her husband in his scientific inquiries or experiments: her health was delicate, and she could not assist him in his out-door observations; but she supplied what was of more importance than these aids—a genial and loving interest ever manifested in his pursuits and successes, and in his very failures. *Alere flammam* was the office of her delicate and poetical temperament. Younger than Mr Espy, she nevertheless died several years before him (in 1850), leaving him to struggle alone in the decline of life without the sustaining power of her devoted and enthusiastic nature.

Having in a great degree matured his theory of storms,—having made numerous inductions from observations, and having written a great deal in regard to it,—Mr Espy took the bold resolution, though past middle age, to throw himself into a new career, laying aside all ordinary employments, and devoting himself to the diffusion of the knowledge which he had collected and increased, by lecturing in the towns, villages, and cities of the United States. This proved a successful undertaking, and by its originality attracted more attention to his views than could have been obtained, probably, in any other way. He soon showed remarkable power in explaining his ideas. His simplicity and clearness enabled his hearers to follow him without too great effort, and the earnestness with which he spoke out his convictions carried them away in favour of his theory. The same power which enabled him to succeed in his lecturing career, procured subsequently for Mr Espy the support and encouragement of some of the leading men in Congress, and especially in the Senate, and also in the executive departments. Their attention was arrested by the originality of his views and his warmth in presenting them, and he imparted so much of his conviction of their truth as to induce many of our statesmen and official persons to exert themselves to procure for him, under the patronage of the government, continued opportunities for study, research, and the comparison of observations. To the consistent support of his scientific friends, and particularly of the Secretary of this Institution, Mr Espy owed also much in obtaining the opportunities of keeping in a scientific career. His reports to the Surgeon-general of the Army, to Congress, and to the Secretary of the Navy, are among his latest efforts in this direction.

The earnest and deep convictions of the truth of his theory in all its parts, and his glowing enthusiasm in regard to it—perhaps, also, the age which he had reached—prevented Mr Espy from passing beyond a certain point in the development of his theory. The same constitution of mind rendered his inductions from observation often unsafe. His views were positive and his conclusions absolute, and so was the expression of them. He was not prone to examine and re-examine premises and conclusions, but considered what had once been passed upon by his judgment as finally settled. Hence his views did not make that impression upon cooler temperaments among men of science to which they were entitled—obtaining more credit among scholars and men of general reading in our country than among scientific men, and making but little progress abroad.

Feeling that his bodily vigour was failing, and that his life must soon close, the Secretary of the Smithsonian Institution induced him to re-examine the various parts of his meteorological theories of storms, tornadoes, and waterspouts, and to insert in his last report, while it was going through the press, an account of his most mature views. I trust that the Secretary will, in one of his reports, give us a thorough and critical examination of the works and services of this remarkable contributor to a branch of science, the knowledge of which the Smithsonian Institution has already done so much to advance and to diffuse.

On the motion of Professor Bache, the following resolutions were adopted:

***Resolved*, That the Regents of the Smithsonian Institution have learned with deep regret the decease of James P. Espy, one of the most useful and zealous of the meteorologists co-operating with the Institution, and whose labours, in both the increase and diffusion of knowledge of meteorology, have merited the highest honours of science at home, and have added to the reputation of our country abroad.**

***Resolved*, That the Regents offer to the relatives of Mr Espy their sincere condolence in the loss which they have sustained.**

On the motion of Mr Pearce, it was resolved that the remarks of Professor Bache be entered in the Proceedings.

PUBLICATIONS RECEIVED.

Experiences of Forty Years in Tasmania. By Hugh M. Hull, Esq. —*From the Publishers.*

First Report of a Geological Reconnoissance of the Northern Counties of Arkansas.—*From the State of Arkansas, through the Smithsonian Institution.*

Description of a Deformed Fragmentary Human Skull, found in an Ancient Quarry-Cave at Jerusalem. By J. Aitken Meigs, M.D.—*From the Author.*

Transactions of the Academy of Science of St Louis. Vol. I., No. 3. 1859.—*From the Academy.*

Smithsonian Report for 1858.—*From the Smithsonian Institution.*

Report of the Geological Survey of the State of Iowa. Vol. I., Parts 1 and 2.—*From the State of Iowa.*

Report by the Superintendent of the United States Coast Survey for 1857.—*From Professor Bache.*

Journal of the Asiatic Society of Bengal, No. 2, 1860.—*From the Editors.*

The Canadian Naturalist and Geologist. August and October 1860.—*From the Society.*

Baxter on Organic Polarity.—*From the Author.*

The Six Months' Seasons of the Tropics. By James Lees.—*From the Publishers.*

Proceedings of the Academy of Natural Sciences of Philadelphia. March and June 1860.—*From the Academy.*

Proceedings of the Literary and Philosophical Society of Manchester. October and November 1860.—*From the Society.*

The Progressive Screw as a Propeller in Navigation. By Julian John Revy, C.E. 1860.—*From the Author.*

Quarterly Journal of the Chemical Society. July 1860.—*From the Society.*

The Typical Character of Nature; or, All Nature a Divine Symbol. By Dr Thos. A. G. Balfour.—*From the Author.*

Species not Transmutable, nor the Result of Secondary Causes. By Dr Bree.—*From the Author.*

ERRATA IN VOL. XII.

The following Errata in Dr Daubeny's paper are owing to his having been absent on the Continent, and not having had an opportunity of revising the proofs when they were passing through the press.

Page 173 line 20 from foot, *for* Professor of Chemistry *read* Professor of Botany.
,, 173 ,, 14 from foot, *for* found *read* formed.
,, 174 ,, 13 and 39 from top, *for* Duperry *read* Dufrenoy.
,, 174 ,, 14, *for* geologist, would take *read* geological world, took.
,, 176 ,, 1, *for* Voilet and Boplarge *read* Virlet and Boplaye.
,, 176 ,, 17, *for* general upheaval *read* ground upheaved.
,, 177 ,, 4, *for* Pestal and Lenz *read* Postel and Linz.
,, 177 ,, 13, *for* march *read* mantle.
,, 178 ,, 13, *for* some *read* none.
,, 178 ,, 31 and 33, *for* Commeni *read* Cammeni.
,, 179 ,, 23, *for* appearances *read* appearance.
,, 180 ,, 8, *for* Caldron *read* Caldera.
,, 180 ,, 20, *for* vicinity *read* viscosity.
,, 181 ,, 36, *for* Mauna *read* Mouna.
,, 182 ,, 11, *for* fast *read* fresh.
,, 182 ,, 18, *for* eruption *read* eruptive.
,, 182 ,, 33, *for* effected *read* affected.
,, 184 ,, 4, *for* to hurry *read* of hurrying.
,, 184 ,, 11 and 14, *for* Ceutal *read* Cantal.
,, 184 ,, 13, *for* these *read* three.
,, 184 ,, 14, *for* Meyer *read* Mezen.
,, 185 ,, 24, *for* 1843 *read* 1848.

THE

EDINBURGH NEW
PHILOSOPHICAL JOURNAL.

Notes on the Silver produced in Cornwall. Communicated by William Jory Henwood, F.R.S., F.G.S., &c.*

It may not be without interest to present at one view the notices of Cornish Silver and Silver ore which have been scattered by many hands through different publications.

"Silver found in Cornwall unmixed (I mean free from tin, copper, or lead), I have never seen but once, and that was found native, about the bigness of a walnut (of which I have part), in Huel-cock, a copper-work in the parish of St Just." (Borlase, *Natural History of Cornwall*, 1758, p. 209).

"In the vein formerly worked at Huel Mexico, the ore appeared . . . as insulated masses or nests, and beside native silver, corneous ore (muriate of silver) was also met with." (Berger, *Geological Transactions*, i. p. 171). Huel Mexico, in Perran Zabuloe, "produced considerable quantities of rich horn-silver, and . . . some fibrous native silver was found in the Gossan."† (Lysons, *Cornwall*, ccx. Carne, *Cornwall Geol. Trans.* i. p. 121). "About thirty years ago, a lode of silver was discovered near the sea, between St Agnes and St Michael, and the mine, which was called Huel Mexico, produced above L.2000 worth of ore."‡ (C. S. Gilbert, *Historical Survey of Cornwall* (1817), i. p. 218. Rashleigh, *British Minerals*,

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* This paper was in type last November, and was intended for the January Number of this Journal, but pressure of matter prevented the Editors from inserting it.—[Ed. *Phil. Journal.*]

† Siliceous earthy-brown iron ore.

‡ My grandfather was one of the unfortunate shareholders in this the first Cornish silver mine.

ii. p. 19, plate xvi.) About fifteen years since, a few small bunches of exceedingly rich silver ore (particularly horn-silver, or muriate of silver, a very rare production) were raised in Cubert parish. Many of these pieces were finely crystallized; but the most beautiful specimen is in the cabinet of John Williams, Esq., and has been represented and described in that elegant work of Mr James Sowerby, the "British Mineralogy," for 1808. (C. S. Gilbert, *Historical Survey of Cornwall* (1817), i. p. 219).

"At Herland in Gwiner, the silver cross-course, between 110 and 142 fathoms deep, afforded for some distance, on either side of the Main (*Manor*) lode, which it intersects, a mixture of galena, native bismuth, grey cobalt-ore, vitreous silver-ore, and native silver; of which about 108 tons were raised. The richest mass of silver ore was found at but two fathoms above the level at which it disappeared." (Hitchin's *Philosophical Transactions*, 1801, pp. 159–163. Berger, *Geol. Trans.* i. p. 171. William Phillips, *Geol. Trans.* ii. p. 152. Lysons, *Cornwall*, ccx. Polwhele, *Cornwall*, iv. p. 134. C. S. Gilbert, *Cornwall*, i. p. 218. Carne, *Cornwall Geol. Trans.* i. p. 121; ii. p. 113. Rees' *Cyclopædia*, Article "Silver," xxxii. De la Beche, *Report on the Geology of Cornwall*, &c., p. 288).

The Quantities of Ore, the Proportions of Silver they contained, and the Sums realised by their Sale, were—

Date.			Weight of Ore.				Proportion of Silver in Ore.	Amount.		
			Tons.	cwts.	qrs.	lbs.				
1799,	Aug.		17	10	0	0	0·005407	£928	6	4
,,	,,	20.	2	0	0	0	0·006120	96	0	0
,,	Sept.	30.	3	1	2	17	0·006334	154	2	6
1800,	Jan.	20.	19	18	0	0	0·006334	995	0	0
,,	,,	30.	1	12	0	0	0·006334	80	0	0
,,	Feb.	20.	3	0	3	25	0·006334	152	8	8
,,	April	5.	20	5	3	13	0·006334	1014	13	3
,,	,,	24.	6	1	0	0	0·006334	302	10	0
,,	,,	26.	12	6	3	0	0·006334	616	17	6
,,	,,	,,	0	4	3	7	0·021236	42	19	3
,,	Aug.	5.	21	18	1	15	0·006334	1095	19	0
,,	,,	,,	1	12	0	0	0·006334	77	5	6
,,	,,	,,	6	4	1	7	......	93	6	0
			115	15	3	0		£5649	8	0

(Alfred Jenkin, Esq., of Redruth, the Landowner's Steward. MSS.)

"Abandoned for a while, the works were resumed in 1825, and when the silver cross-course, wrought to a greater depth, again afforded small quantities of silver ore." (Henwood, *Cornwall Geol. Trans.* v., Table xxxiv.), viz.:—

Date.	Weight of Ore. Cwts.	qrs.	lbs.		Proportion of Silver.
1833, Feb	4	2	0	...	0·012546
" Aug.	7	1	0	...	0·001714
" "	4	2	0	...	0·004162
" "	2	2	0	...	0·006334
" "	0	2	0	...	0·006946
	19	1	0		

(Edward Michell, Esq., of Penpol Smelting Works, MSS.)

At the adjoining mine, "Huel Alfred, in 1813, some native silver was found, appearing as if plated on the copper-ore, but in so small a quantity as not to be worthy of notice." (Carne, *Cornwall Geol. Trans.*, i. p. 123). "Native silver is accompanied by red, grey, and black silver-ore, in Huel Alfred." (William Phillips, *Mineralogy*, third edition, p. 285). "Huel Alfred, in Phillack, has produced native silver in green carbonate of copper." (Michell, *Manual of Mineralogy*. Truro 1825, p. 20.)

"Huel Ann, in Phillack, furnished in 1814 a small bunch of blackish-grey silver ore, containing a great deal of arsenic and spathose iron, and accompanied by native silver, at the sixty-five fathoms level, in an east and west copper lode. In the seventy-five fathoms level, the silver ore appeared in the midst of the copper lode, as a separate lode from two to five inches wide. . . . Some fine specimens of fibrous native silver were found in this level; but these, as well as the ore, proved of very short duration." (Carne, *Cornwall Geol. Trans.*, i. p. 124; ii. pp. 105, 120). "Huel Ann has produced native capillary silver, with surprising fineness of fibre, in tin-white and grey cobalt, and grey silver, in arsenical pyrites." (Michell, *Manual of Mineralogy*, p. 20). "In Dolcoath, in 1810, some grey silver ore, intimately united with cobalt ore, together with a little native silver, were found in the sixty fathoms level, in the copper lode, very near a small cross-course." (Carne, *Cornwall Geol. Trans.*, i. p.

122. William Phillips (*Mineralogy*, third edition, p. 288. Michell, *Manual of Mineralogy*, p. 20). "In the South Entral Lode at Dolcoath, much native silver, as well as vitreous and red silver ore, were mixed with galena, blende, and with still larger quantities of iron and copper pyrites, quartz, and slaty clay." (Henwood, *Cornwall Geol. Trans.*, v. p. 66, Table 1.)

The Silver Ores sold from this part of the Mine were:—

Date.	Weight of Ore. Tons.	cwts.	qrs.	Proportion of Silver.	Amount.		
1833, Nov. 23.	1	15	1	0·011995	£175	8	2
„ „	1	0	3	0·002478	17	6	4
1834, July.	4	4	0	0·008476	290	2	0
„ „	3	3	0	0·004039	96	4	6
„ Aug. 21,	4	16	0	0·006273	239	10	8
1835, Jan. 6.	3	3	0	0·007650	194	5	0
„ „	4	19	0	0·005202	200	7	1
„ „	1	13	0	0·003978	49	8	4
„ „ 12.	2	12	2	0·005875	121	18	9
„ „ „	4	0	1	0·002846	80	14	6
1836, Jan. 12.	1	10	3	0·002968	32	4	2
„ „	0	12	2	0·006334	31	10	0
„ „	0	4	1	0·049909	91	8	2
„ Nov. 23.	1	12	3	0·002111	21	16	8
„ „	2	4	2	0·001928	26	9	8
„ „	0	17	0	0·003764	23	15	7
„ „	0	18	0	0·000734	1	6	6
„ „	0	11	0	0·010220	46	7	1
	39	17	2		£1740	3	2

Beside which, more than one thousand three hundred pounds were realised from ore smelted at the mine. Entral South Lode has therefore yielded more than three thousand pounds worth of silver." (Captain Charles Thomas, manager of Dolcoath Mine, MSS.) "The noble family of Basset, lords of the land, preserve as an heirloom at Tehidy, a splendid piece of plate manufactured of silver raised at Dolcoath, which was presented to the late Lord de Dunstanville, by the adventurers in that mine, as a testimony of their gratitude for his liberality in relinquishing his *Lord's dues*, whenever the poverty of the concern rendered further outlay necessary." (Henwood, *Cornwall Geol. Trans.*, v. p. 66, *Note*.)

"The silver ore of North Dolcoath, an adjoining mine, afforded, during 1859–60, 0·003011 its weight of metal." (Captain Joseph Vivian, manager of North Roskear Mines, MSS.)

Mr Carne states, that at Huel Basset, in Illogan, a small cross-course, traversing the lode, afforded in 1813, vitreous silver-ore, some of which yielded (0·018360 its weight) six hundred ounces of silver per ton, at and within about six feet on either side of the intersection (*Cornwall Geol. Trans.*, i. p. 124); and Mr William Phillips observes, that silver ore worth three thousand pounds was obtained there. (*Mineralogy*, third edition, p. 236). Mr Martyn, the sole survivor of those who at that time, and long afterwards, were agents at Wheal Basset, says, that during his connection with that mine, silver-ore was discovered but once,—that the quantity was very trifling, the quality such that for several years no purchaser could be found, —and at length it realised only a small sum. He adds, that there was neither record nor tradition of any similar discovery having been made previously. The late John Williams, Esq., of Scorrier House, had the entire direction of Wheal Basset, during this period. His account-books, examined in aid of this inquiry, fully confirm the accuracy of Mr Martyn's recollection; and show that the silver ore raised in 1813 was not disposed of until 1827, when it was sold for L.15, 12s. 4d. only.

"Silver ore is thinly dispersed through (*gossan*) siliceous earthy-brown iron ore, in the shallower parts of Treskerby, near Redruth. In 1827, a parcel of it realised L.15, 14s. 4d. At the same time a smaller quantity of similar ore from the adjoining mine of North Downs was sold for L.3, 4s. 9d." (William Williams, Esq. of Tregullow, MSS.)

The ancient mine of Bal-Dhu, near Truro, affords enormous masses of the same ore, which have been, from time to time, largely wrought for the scattered particles of silver ore they contain.

"Huel Duchy, in Calstock . . . one of the principal silver mines . . . has already produced about L.4000, and promises to be yet more profitable. (Lysons, *Cornwall*, ccx.) "Wheal Duchy, in Calstock (discovered in searching for copper), has been worked with success. The whole lode is from 6 to 12 inches wide; the part containing silver from 1 to 4. It runs east and west—the direction of other similar veins in the neighbourhood. Some of the ores contain from 60 to 70 parts in 100 (0·6 to 0·7) of pure silver. About L.5000 worth of silver, a year or two ago, had covered expense of the undertaking in its early stages." (Polwhele, *Cornwall*, iv. (1816), p. 134.) Some silver works are still kept open at Calstock by the name of Wheal Duchy, from which the cup presented to the Duke of Cornwall, in the year 1812, by Benjamin Tucker, Esq., was extracted." (C. S. Gilbert, *Cornwall*, i. p. 219). "At Huel Duchy, near Callington, in a lode inclining to the north-east and south-west, and from one to three feet wide, in which the adventurers were searching for copper, detached lumps of silver ore and small bunches of native silver were found at the adit. In the ten-fathoms level there was a regular course of silver ore, accompanied by native silver, for nearly three fathoms in length, yielding above L.200 worth per fathom. This was its richest part. In the twenty-fathoms level some native silver and bunches of silver ore were found, but the lode had there declined in value. In the next deeper level the silver was exhausted. The ore consisted chiefly of red and grey silver and black oxide of silver. The value of the silver produced was about L.3000." (Carne, *Cornwall Geol. Trans.* i. p. 122.) After this the mine remained many years unwrought, but in 1833 it was reopened under the name of Wheal Brothers. (De la Bèche, *Report on Cornwall, &c.*, p. 613. Henwood, *Cornwall Geol. Trans.* v. p. 140, table xciii.) "Portions of argentiferous earthy brown iron ore (silver-gossan) left by the former adventurers in shallow parts of the mine, were now extracted and sold for between L.150 and L.200 per ton. But so greatly did the ore differ in quality, that some parcels of it brought no more than L.2, and others, containing 0·059976 their weight of silver, as much as L.500 per ton in the market" (Captain Knott, of Calstock, MSS.) "At the thirty-fathom level, masses of native silver, weighing many pounds each, occurred in a part of the *lode* which was worth from L.500 to L.600 per fathom. But great part of the ore, when brought to the surface, contained only eight to ten ounces (0·000245 to 0·000306, its weight), although when (*dressed*) prepared for sale, it yielded sixty ounces (0·001836 its weight) of silver per ton." (Percival Norton Johnson, F.R.S., F.G.S. MSS.)

"Wheal Sisters, opened also in 1833, on an eastern part of the same lode, afforded ore which, resembling for the most part that of Wheal Brothers, contained from 0·000459 to 0·07344 its weight of silver. Portions of it were, however, mixed with from 0·15 to 0·20 their weight of lead. Small quantities of the silver ore were sold at twenty shillings a pound; and some parcels found a market at from L.400 to L.500 per ton, but the ordinary price was between L.30 and L.50." (Captain Knott, an agent of the mine. MSS.)

Wheal Saint Vincent yielded great quantities and many varieties of silver ore from a parallel lode south of that wrought at the same time in the neighbouring mine of Wheal Duchy; but, no longer affording profit, it was closed in 1824. If accounts of the produce still exist, they are inaccessible.

In 1835, the works then named the East Cornwall Silver Mines were reopened, but after an unsuccessful trial of about two years, they were again abandoned. During that period they yielded the undermentioned ores:—

Date.	Weight of Ore.				Proportion of Silver.
	Tons.	cwts.	qrs.	lbs.	
1857, Sept. . . .	0	17	1	0	0·002708
" " . . .	0	6	3	0	0·009118
" " . . .	0	4	0	14	0·030263

(Edward Michell, Esq. MSS.)

"The same mine was opened a third time in 1848 under the name of Wheal Langford, but silver ore became less and lead ore more abundant as the works were deepened. For several years the ore afforded from 0·001224 to 0·060466 its weight of silver, and its price ranged between L.10 and L.550 per ton. A parcel weighing 2 tons 6 cwts. 2 qrs. 6 lbs., realised in June 1855 L.1184, 15s. 9d." (Captain Knott, an agent of the mine. MSS.)

"Wheal Mexico, wrought to an inconsiderable depth on an eastern part of the same lode, during 1847–8, afforded the chloride of silver largely mixed with slaty clay, granular quartz, and the carbonate of iron. Some portions of the ore contained 0·001224; others, 0·026316, their weight of silver. The prices ranged from L.5 to L.200 per ton; but small quantities free from impurity were sold at L.2, 10s. per (avoirdupois) pound." (Captain Knott, manager of the mine. MSS.)

"About four miles to the south-east of Callington, a silver mine situate in a rock of killas or chloride-slate. The vein was first worked for copper, but native silver and lead ore were discovered in it. The mine is called Huel Jewel; the thickness of the vein rarely exceeds three or four inches. In many of the cavities were found a considerable quantity of capillary native silver, with galena, red silver ore, and sulphuret of silver. The ores were exceedingly rich, and promised . . . an ample recompense to the adventurers." (Rees, *Cyclopædia*, xxvi.; "Silver.")

"In Willsworthy mine, on the border of Devon, the lode is about twelve inches wide, bearing N.N.E. and S.S.W., and underlies two feet and a-half per fathom to the south. In the ten-fathoms level, a vein of white and amethystine quartz divided the lode; between this vein of quartz and the lower or northern wall of the lode, was found a vein of rich arsenical cobalt ore, combined with native capillary silver, in a ferruginous matrix from three to six inches wide. The space between the vein of quartz and the higher or southern wall of the lode was occupied by a vein of rich yellow copper ore from six to nine inches wide. The silver continued about two fathoms in length. The copper was not so soon exhausted. The specimens of native silver from this mine have eclipsed all that have ever before been found in Cornwall, both in size and beauty." (Carne, *Cornwall Geol. Trans.* i. p. 124. William Phillips, *Mineralogy*, 3d edition, p. 285. Mitchell, *Manual of Mineralogy* (Truro, 1825) p. 21. Greg and Lettsom, *Manual of Mineralogy* (London, 1858), p. 240.)

"Native silver, dendritic and in elongated octohedral crystals, and also capillary native silver on arragonite, have been obtained from Levant near Penzance." (Carne, *Cornwall Geol. Trans.* vi. p. 48. Garby, *ibid.* vii. p. 87.)

Arsenical silver ore has been discovered in quartz near Camborne; and native silver in copper pyrites at Crennis near St Austell; in siliceous earthy-brown iron ore (*gossan*), at the Fowey Consolidated Mines near Fowey; and in galena at Wheal (Providence); Tremayne, near Hayle; but in each of these instances the quantity has been very small. (Michell, *Manual of Mineralogy*. Truro, 1825, p. 21. Garby, *Cornwall Geol. Trans.* vii. p. 88. Greg and Lettsom, *Manual of Mineralogy*. London, 1858, p. 240.) Between Marazion and Penzance, West Wheal Darlington was wrought, in search of copper ore, to nearly forty fathoms from the surface, and some twenty-five below the sea, in closely-jointed, deep blue, homogeneous clay-slate, on a lode which, bearing 10° to 20° N. of E. and S. of W., and composed of quartz, quartzose slate and iron pyrites,—unexpectedly yielded large masses of native silver, mixed sometimes with galena, and less frequently with copper pyrites.

The following are particulars of its proceeds:—

Date.		Weight of Ore.				Proportion of Silver.	Amount.		
		Tons	cwts.	qrs.	lbs.				
1852,	Nov. 9.	0	14	0	0	0·032100	£179	7	6
„	„	0	18	3	0	0·008048	67	2	6
„	„	1	4	0	0	0·001805	11	2	0
„	„	0	9	0	0	0·003825*	15	1	6
1853,	Jan. 31.	3	1	1	0	0·004957	88	19	2
„	„	4	19	1	15	0·001224	37	17	7
„	March 29.	1	18	3	20	0·002815†	75	11	10
„	May 10.	0	4	3	14	0·060282	127	13	2
„	„	3	9	2	1	0·007497	177	12	0
„	„	1	5	0	16	0·003978	20	2	3
„	„	1	18	3	17	0·003672	49	2	2
„	„	1	9	1	26	0·002142	22	16	9
„	July 19.	0	5	2	22	0·076500	180	15	10
„	„	3	7	0	15	0·005141	123	1	10
„	„	2	10	2	20	0·004896	81	11	10
„	Aug. 18.	0	11	0	19	0·030294	170	6	10
„	„	1	2	0	21	0·001867‡	22	8	1
„	„	3	4	0	11	0·002815	62	16	2
„	„	9	2	0	5	0·002509	121	1	2
„	„	4	6	1	24	0·001254	28	2	3
„	„	1	10	2	21	0·000612	4	12	0
„	Sept. 28.	0	8	2	11	0·112118	462	5	2
„	„	2	8	0	11	0·004039	85	19	6
„	„	3	18	3	12	0·001867	42	9	7
Carry forward,		54	8	3	21		£2257	18	8

* Beside 0·32250, its weight of lead.

† „ 0·630000 „ „

‡ „ 0·570000 „ „

Date.	Weight of Ore. Tons	cwts.	qrs.	lbs.	Proportion of Silver.	Amount.		
Brought over,	54	8	3	21		£2257	18	8
1853, Sept 28.	7	9	1	10	0·002867	141	17	5
„ „	3	16	1	5	0·002509	48	12	8
„ Nov. 9.	0	9	3	2	0·039351	182	0	11
„ „	11	14	3	0	0·001254	94	15	7
„ „	10	5	3	0	0·001560	81	10	6
„ „	7	5	3	22	0·001060	38	6	3
„ „	2	0	2	0	0·000627	4	1	0
„ Dec. 20.	2	0	0	0	0·002172		...	
„ „	6	0	0	0	0·001224		...	
„ „	10	0	0	0	0·000765		...	
„ „	15	0	0	0	0·000612		...	
„ „	5	0	0	0	0·000551		...	
„ „	2	10	0	0	0·000306		...	
1854, Feb. 22.	2	7	0	25	0·002509	57	17	0
„ „	17	7	0	21	0·001163	125	17	1
„ April 11.	5	5	0	0	0·001652	86	12	6
„ May 25.	1	13	2	12	0·002540	30	13	3
„ „	7	8	1	6	0·001224	50	1	0
„ July 19.	0	16	2	12	0·143820*	53	19	4
„ „	0	12	1	18	...	14	2	4
„ „	4	2	1	16	...	32	2	9
„ „	1	17	0	7	...	7	17	6
„ Aug. 30.	0	17	3	27	0·000964†	18	1	8
„ „	5	18	2	0	0·001224	75	3	6
„ „	2	3	1	14	0·000918‡	24	17	3
„ Nov. 15.	4	1	1	0	0·001102	24	7	6
„ „	2	14	3	0	0·000612	7	3	4
„ Dec. 31.	52	4	0	0	0·000306	150	18	0
„ „	18	19	1	0	0·000409			
„ „	79	14	3	0	0·000153			
1855, Feb. 14.	0	0	0	7	...	16	0	0
„ „ 24.	2	3	1	0	0·001316§	27	0	7
„ „	1	5	0	0	0·000153‖	7	7	6
„ „	2	0	3	0	0·001163		...	
Quantity of ore sold,	309	3	1	1	Value of ore sold,	£3659	5	1
Unsold,	42	10	3	0				

* Beside 0·335000 its weight of lead.

† „ 0·436250 „ „

‡ „ 0·060000 „ „ copper.

§ „ 0·290000 „ „ lead.

‖ „ ·062500 „ „ copper.

Highest price of ore,	£5120	0	0	per ton.
Lowest	1	0	0	,,
Average	11	16	8	,,

(Compiled from the Mine-books by favour of W. J. Rawlings, Esq. of Hayle.)

At Trebisken-green, near Truro, a lode oblique in direction to the large iron vein so well known in the same neighbourhood (Henwood, *Cornwall Geol. Trans.* v. p. 108), affords irregular masses of rich ore. This, for the most part, is galena, which sometimes contains no more than 0·000153, but in some cases has yielded as much as 0·091922 to 0·104040, and even 0·122584 its weight of silver. Portions of the lode, however, have produced 0·107100 their weight of silver from vitreous silver ore and native silver unmixed with lead.

The ore sold from the mine has been—

Date.	Weight of Ore. Tons	cwts.	qrs.	lbs.	Proportion of Lead.	Amount.		
1859, Sept. 14.	1	14	2	0	0·750000	£164	15	6
,, ,, 26.	0	2	0	0	0·450000	7	8	4
1860, Mar. 13.	0	1	3	16	0·750000	41	4	4
,, ,,	0	14	3	19	0·650000	100	14	1
,, ,,	0	4	3	11	(Slime-ore)	1	16	4
,, Aug. 14.	1	9	0	12	0·650000	485	3	6
,, ,,	2	19	0	1	0·612500	286	3	10
	7	6	1	3		£1087	5	11

Assays prove the average proportion of silver to have been 0·016189 the weight of ore, and 0·024590 the weight of lead.

(Edwin Carter Esq., purser of the Mine and Edward Michell, Esq. of Penpol Smelting Works. MSS.)

The lead ores of Cornwall are proverbially rich in silver, but a description of them is foreign to the object of this communication.

P.S.—Since the foregoing particulars were compiled, the Rosewarne and Herland mines, wrought in clay-slate, within short distance of Herland, have yielded, at the thirty fathoms level, from a portion of the *cross-vein*, no more than fifteen feet in length by six feet in height—

Tons.	cwts.	qrs.	lbs.					
2	2	0	0	of silver ore, which was sold at	£27	8	0	per ton.
0	19	0	0	,, ,,	77	10	0	,,
0	6	0	0	,, ,,	65	7	6	,,

Royal Cornwall Gazette, 18th January 1861.

Organogenic Researches on the Female Flower of the Coniferæ.* By Dr H. BAILLON. Memoir presented to the Académie des Sciences at its Meeting, 30th April 1860. Translated by ALEXANDER DICKSON, M.D., Edinburgh.†

There are various modes of viewing the organisation of the flowers of the Coniferæ. I do not intend to recount here, in detail, all the different interpretations. The history of these has been given elsewhere by several botanists, and more especially by the illustrious R. Brown (*App. to Captain King's Voyage*, and *Ann. des Sc. Nat.* 1826, p. 236). But on going over the most prominent points in the different views, we may classify them under five principal heads.

1. A. L. de Jussieu, and with him Smith and Lambert, admit that there is in the Coniferæ, as in all other flowers, an ovary and an ovule. But here, instead of presenting the form of a sac, the ovary, according to these authors, has two loculi spread open, and its style is represented by a scale placed externally or below. It is worthy of remark that this view admits, at the same time, a duality of carpels.

2. A second opinion, instead of considering the ovary as presenting an exceptional organisation, attributes to it the form which it usually possesses in the vegetable kingdom, viz., that of a sac inclosing the future seed. Such is the interpretation proposed by M. Blume. But the learned Dutch botanist does not admit that it can be applied to all the Coniferæ. According to him, it holds true for the Taxineæ and Gnetaceæ, "the carpel of which is urceolate," but not for the Abietineæ and Cupressineæ, in which "the ovary is not closed, and where the ovule is directly exposed to the action of the pollen." (*Rumphia*, iii. 208; iv. 2.)

What M. Blume applies only to certain Coniferæ, has, on the other hand, been considered as applicable to the entire order by Messrs Mirbel and Spach.* In their excellent paper on the embryogeny of these plants (*Ann. des Sc. Nat.* 2d ser. xx. 259), they consider the flower as consisting "of a conical nucleus contained within an open ovary."

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* I have given a nearly faithful translation of the original, only making omissions in some inessential particulars.—A. D.

† Translation read before the Botanical Society of Edinburgh, 10th January 1861.

3. According to a third view, the flower comprises an ovary and ovule, and is further provided with a perianth. Such appears to have been the first opinion of Mirbel, when he admitted with Schubert (*Nouv. Bull. des Sc.* iii. 73, 85, 121), that the gynœcium is accompanied by a small adherent perianth, besides an accessory external envelope or cupule. This opinion was also held by R. Brown in his account of Flinders' Voyage (ii. 572).

L. C. Richard considered the Coniferæ as provided with a pistil, and with a simple perianth or calyx (*Mém. sur. les Conif. et les Cycad.* 96), and A. Richard, adopting his father's ideas, describes the female flowers as presenting a "gamosepalous calyx adherent to the ovary, which is either partially or wholly inferior, and a unilocular ovary containing a single ovule" (*Elém.* édit. 7^{me}, 657).

4. R. Brown, on reconsidering his first interpretation (*l. c.*), and comparing what at that period was termed the ovule of the Coniferæ with that of other phanerogamic plants, imagined that this ovule was much too simple when compared with all the others which he had studied, and from this comparison originated what is called the Gymnospermous theory. Ever since, the ovule of the Coniferæ has been declared to be naked, and its placenta to be of a foliar nature. It was by an error that physiologists had, until that time, taken the integuments of this ovule for floral envelopes, and considered its summit to be a style.

5. In our days, says Dr Lindley (*Veget. Kingdom*, 227), every one agrees as to the correctness of R. Brown's views. Schleiden also admits the gymnospermous character of the Coniferæ; but making a rigorous application to these plants of his remarkable principles on the axial nature of the placenta, the illustrious *savant* regards the organ bearing the

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* The first indication of this view is found in the "Eléments de physiologie végétale de C. F. Brisseau-Mirbel," vol. i. p. 347, &c., and is reproduced in his principal memoirs.

ovules as the axillary bud of the carpellary leaf (*Ann. des Sc. Nat.* 2d ser. xii. 374); a happy and fertile interpretation, supported by proofs which are really incontestable.

The foregoing shows the multiplicity of the opinions which have been set forth. The gymnospermous theory, in spite of its involving what is exceptional and abnormal, seems to be generally adopted at the present day. Such is the opinion of all classical authors and all the elementary treatises. "Certain ovules are 'naked,' says Ad. de Jussieu (*Cours Elém.* édit. 1re, 446), and we may term those plants which possess them Gymnosperms." Endlicher (*Genera*, 258), in speaking of the Coniferæ, says, "*ovula nuda*;" and Dr Lindley (*l. c.* 227) includes them with the Cycadaceæ and Gnetaceæ in his class of Gymnogens. It is nevertheless allowable to apply to the verification of these opinions the new means of investigation which organogenic study affords us. This is what I have attempted, and the observations which I have made on these interesting plants are already very numerous. I shall only, however, present some of the principal results, setting forth, in the first instance, such facts as are met with in the species most widely distributed around us, so that it may be easy to verify my observations.

A. In the axils of certain leaves in *Taxus baccata* (the Yew), there are developed little floriferous branches. Those bear at first a considerable number of nearly decussate and imbricated bracts. The summit of the little axis which bears them serves for the floral receptacle. This, in fact, is soon seen to produce two little mammillæ, curved in the form of a horse-shoe, which unite to form a kind of horizontal ring. This is nothing else than the first rudiment of an ovary. It grows into the form of a conical sac; its aperture, which is directed upwards, is nevertheless always seen to be divided into two slightly marked lips or lobes; they alternate with the two leaves or scales which precede the pistil. Of a similar appendicular [foliar] nature, they continue the series of decussation. The only difference consists in the early connation of the two leaves which constitute the ovary.

In proportion to the elevation of the sac, the summit of the axis becomes elongated to form the first rudiment of the ovule.

I believe that the axial nature of the support of this ovule has not been contested by any one.

B. Phyllocladus rhomboidalis (Rich.) has a small number of solitary female flowers, borne in the axils of little leaves or bracts upon a common axis. Each one of these flowers, which is quite free from its accompanying bract (*bractée axillante*), becomes developed above it, and consists of a little dome-like receptacle bearing two laterally connate carpellary leaves, quite similar to those of the Willow or Yew. From the bottom of this enclosure an erect ovule is afterwards developed. Later, but before the period of impregnation, the base of the gynœcium becomes surrounded by a projecting circular pad (*bourrelet*), similar to what is termed the aril in *Taxus;* it is a disk, such as is so often elsewhere developed, at a late period, at the base of the ovary. Thus it appears that *Phyllocladus* is a *Taxus* in which the female flowers are axillary and solitary instead of being terminal.

C. In *Torreya nucifera* (Siebold) a bud appears in the axil of a leaf, and becomes developed into a little branch, bearing alternate leaves. Of these leaves, the inferior and superior remain sterile, but some of the intermediate ones are fertile. In the axil of each of the latter, a large undivided and rounded mammilla appears, which is at first somewhat pointed at its summit. The summit soon becomes depressed, and two lateral bracts appear to right and left of the mammilla. The mammilla then commences to bifurcate (*se dédoubler*). Its summit having at first a plane surface (*coupé d'abord horizontalement*), afterwards presents a vertical median furrow, which becomes daily more pronounced. A single axis in this axil is thus found to be replaced by two collateral axes. Each of these produces two pairs of little decussate bracts, which appear from below upwards; and above these two carpellary leaves, which quickly become connate, like those of the Yew, and, by their growth, form a sac with a nearly circular aperture, and surrounding an erect orthotropal ovule. Thus *Torreya* is a *Taxus*, with this difference, that here the axillary floriferous branch always bifurcates, which only occurs as an exception in the Yew.

D. In *Thuja*, as in *Torreya*, we have two female flowers in the axil of each bract. Above the sterile bracts, the axis of inflorescence becomes slightly tumid in the axils of the fertile bracts, and produces two little dicarpellary flowers, like those of the Yew and *Phyllocladus*. An orthotropal ovule is also similarly developed upon the basilar placenta. As to the wings, which soon appear on the young fruit, they are expansions of the dorsal rib of the two carpels. A *Thuja*, then, differs from a *Torreya*, in its flowers being sessile, and not being preceded by any bracts below the carpellary leaves; and from *Phyllocladus*, in a pair of flowers, instead of one only, being borne by the axis above each leaf.

E. In its earliest condition, the cone of a Pine, *Pinus resinosa* for instance, is represented by a cylindro-conical axis with an obtuse summit. There are developed upon its surface a great number of alternate bracts. Like the leaves which every other branch bears, these bracts take on an unequal development, so that those at the base, which are often sterile, and those at the apex, are soon smaller than those towards the middle of the cone. These are *the only appendicular* [foliar] *organs* found upon this cone, and their development, as we shall presently see, becomes arrested at an early period.

In the axil of each of these bracts there is soon produced a little branch, which for a long time is represented by a cellular mammilla, obtuse, and flattened (*surbaissé*), compressed from without inwards between the main axis and the accompanying bract. When we know the great readiness with which all receptacular axes in plants become modified in form (*se déforme*), we are not surprised to see this, which is destined to bear the flowers, undergo the changes in form which are now to be described.

Its summit exhibits at first two small shallow depressions (*échancrures obtuses*), by which it is divided into three lobes, a middle and two lateral. The middle lobe, which forms a kind of apiculus, is the organic apex of the axis. The two lateral become gradually enlarged into the form of auricles, upon the lower slightly decurrent portion of which the female flowers are to be borne. Before, however, occupying ourselves with the appearing of these latter, let us notice what becomes of the middle lobe. In consequence of a constant modification in form (*déformation*), which gradually operates in the little axis, the median lobe ceases, by degrees, to occupy the apparent summit; it is gradually thrown back from the edge of the internal surface, until, at last, it nearly occupies its centre. Moreover, it frequently occupies the interval between the two scales placed above it, and this contributes in no small degree to give it the narrow and elongated form which it ordinarily presents.

What we first see of the female flower is a pair of little carpellary leaves, in the form of a horse-shoe, with their concavities presented to one another. Their summits appear first, and they are at first quite free from each other, but in proportion as these become elevated above the receptacle they become connate by their bases, and thus form a little projecting girdle, which, when viewed from above, appears elliptical. At the centre is found the receptacle which they enclose, and which for some time remains nearly flat. Let us describe exactly the position of this little carpellary enclosure. Its superior aperture is directed to the side, and at first a little downwards; later it becomes altogether inferior; changes of direction which are due to the continuous progress of the modification in the form of the axis.

Whilst the two united carpellary leaves become elongated, so as to form an ovarian sac, continuous at its lower part, and divided higher up into two equal or unequal style-branches, which always recal the duality of the carpels, the axis becomes swollen (*se gonfle*), at the centre, so as to produce the ovule. This last becomes slowly elevated in the form of a dome, without effecting at first any connection with the carpellary leaves. Thus it appears that the ovule here is nothing else than the prolongation of the floral receptacle, and that the placentation is basilar, as in the Juglandaceæ, Polygonaceæ, and Salsolaceæ.

From the foregoing, we see that the Pine only differs from *Torreya*, in the form of the axis, which bears the female flowers, and by the gradual inversion of the latter.*

F. The female flowers of *Salisburia Ginko* (Smith), are borne, two or more, upon the divided extremity of a little branch, as in the Pine; with this difference, that the floriferous axis is rounded and more elongated, and is placed in the axil of a true leaf.

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* I ought to state that, since I made these observations on the female flower of *Pinus resinosa*. Professor Payer has repeated them, and has obtained similar results.

G. The last type which I shall examine here, is presented by the *Cypress.* The axis of the inflorescence (*l'axe du rameau floral*) comports itself exactly as in *Thuja* and *Salisburia*, and the bracts do not bear any other organ. Above each of these the axis becomes slightly and uniformly elevated, it then gives origin, at a considerable distance from the insertion of the scale [bract], and opposite to its median line, to a little dicarpellary pistil resembling that of *Thuja.* Soon, however, another similar flower appears, anterior to, and below the first, with two others at the sides; then others, still more external and inferior, surround the first in several excentric circles, and the number of these may be very considerable. We have here an axillary centrifugal floral group, somewhat comparable to the axillary glomeruli of the Labiatæ, which collectively form a spike, as in this instance.

It will now be easy to submit to the test of the preceding observations the principal objections which have been advanced against our mode of interpreting the floral organisation of the Coniferæ. This will give us room, at the same time, to examine the facts which have been adduced in favour of the opposing theories; and the only thing which, for the moment, I shall pass over in silence, is the comparison of the Coniferæ with Cycads and Gnetaceæ. In the examination which I intend to make of the entire gymnospermous theory, I shall soon have occasion to return upon the two latter orders; and I may say beforehand, that the proofs which may be deduced from their floral organisation are not of any importance in my eyes in the demonstration of the facts which now occupy us.

Robert Brown is astonished that any one can regard a sac perforated at its summit as an ovary, because ordinarily the ovarian pouch is completely closed. But the phenomenon of complete occlusion, although usual, is certainly not without exception. There are plants such as the Cistaceæ, Tamariscineæ, &c., where it is by the superior orifice of the ovary that the pollen-tube penetrates into the ovarian cavity, and this has long been remarked in the open ovary of the Resedaceæ, &c.

The second objection of the illustrious British botanist is of no greater weight. What we, with so many others, call an ovule, appeared to him not sufficiently complicated in structure to be considered as such. But we are all aware that the ovule is not necessarily composed of a nucleus and its two envelopes. Brongniart, Decaisne, and R. Brown himself, have long ago described plants in which the ovule is reduced to its nucleus alone. Botanists have too often confounded in the Abietineæ the scales of an appendicular [foliar] nature, which are borne by the axis of the cone, with the organs situated in the axils of these leaves, to which, from their appearance, the same name has been applied. But henceforth this confusion should be so much the more easily prevented, since Phytotomists are every day attaching less importance to the consistence, form, and colour of an organ, when determining its signification. That which bears the female reproductive organs in the Abietineæ is a body placed in the axil of a leaf (*appendice*); it is then a branch—an axial production—and in relation to this Schleiden has opened the way to an exact determination of the nature of the floral supports. It is incontestable that the axiom "*folium in axillâ folii* is without example in the vegetable kingdom," holds true in this, as in every other case. We should not then be arrested by the objection which Dr Lindley addresses to Schleiden. (*Veget. Kingd.* 2d edit. p. 227.) It is begging the question to consider the fruit of a Willow as "a leaf placed in the axil of another leaf." For even admitting that the placentas of a Willow are formed by the borders of the carpellary leaves, nevertheless there are here two leaves united so as to form the ovary, and they represent a bud with two leaves, to which a receptacle or common support is certainly necessary of an axial nature, let it be as short as one chooses. The comparison which some would establish between the flowers of the two sexes proves nothing in favour of the foliar nature of the body supporting the seeds in the Abietineæ; for the stamens are not, as Dr Lindley plainly says, "the analogues of the indurated carpellary scales of the females." These supposed carpels are, in fact, placed in the axils of leaves. The stamens are not in the same position; they are leaves or modified bracts, which neither have anything in their axils, nor are in the axils of any other organs. In their origin, they resemble the ordinary bract; later, their tissue swells out, and becomes gradually modified at those points which become anthers. But at any age they are not the less of the nature of leaves (*appendices*), which is the character of the stamens; whilst in every pistil, here as elsewhere, there are two distinct portions, the one axial, the other appendicular [foliar].

As to the teratological facts, I would say that they may be made to furnish arguments in favour of the most opposite opinions. In this particular instance, however, there is nothing surprising in the cone of an *Abies* being capable of bearing leaves analogous to those of the branches (Richard, *Mém. Conif.* pl. xiii.), since this cone bears, normally, bracts or appendages, which are nothing but modified leaves.

Conclusions.

1. The female flowers of the *Coniferæ* differ very little in essentials from one another. They are constructed upon a single type, and were we only to regard these, it would be unnecessary to preserve the divisions of the order of the Coniferæ into *Pinaceæ* and *Taxaceæ*.

2. The female flower is either terminal, or placed in the axil of a bract or leaf. But it is always (as Schleiden holds) borne by an axis, and never by a bract. Only the form of this axis is very variable; but this is a characteristic of receptacular organs.

3. Thus, as was believed by Mirbel and Spach, this flower is not gymnospermous, but possesses a dicarpellary ovary, without floral envelopes, and containing an erect orthotropal ovule upon a basilar placenta.

4. The cupule, of variable form and consistence, which surrounds this ovary, and which in several genera has received the name of aril, is a later production, although anterior to fecundation, as is the case with all those floral organs, resulting from a secondary expansion of the axis, which we call disks.

[A selection from Dr Baillon's numerous and very beautiful figures is given in Plate V.]

DESCRIPTION OF PLATE V.

Pinus resinosa, L.

Fig. 1. A bract (*b*) detached from the cone, with its axillary scale (*a*).

Fig. 2. The same, further advanced; the scale (*a*) now presents two lateral lobes or auricles, and a median lobe or apiculus (*ml*), the termination of the axis of the scale.

Fig. 3. Appearance of flowers upon the axis (*a*); each of them composed of two carpellary leaves (*c*) of a crescentic form, between which the placenta (*p*) appears.

Fig. 4. The same, further advanced; the carpellary leaves have become connate.

Fig. 5. One of the flowers from fig. 4 isolated and more highly magnified; *c*, carpels; *ol*, ovule, which appears as the prolongation of the floral receptacle.

Fig. 6. Scale much further advanced; the stylary portions of the two carpellary leaves of the ovary (*ov*) are beginning to become unequally developed.

Fig. 7. Flowers, nearly adult, and borne upon the scale, which in length now nearly equals its accompanying bract.

Fig. 8. Longitudinal section of an adult flower; the styles (*st*) are still more unequal than in last figure: *ov*, ovary; *ol*, ovule.

Cupressus sempervirens, L.

Fig. 9. Young flower-bearing branch. The lower bracts (*sb*) are sterile; the upper bracts, one of which has been cut away (*cb*), have a centrifugal inflorescence (*i*), developed in their axils.

Note upon the preceding Translation, with Observations upon the Morphological Constitution of certain Abietineous Cones. By A. DICKSON, M.D., Edinburgh.*

It is with no little pleasure that I have taken this opportunity of bringing Dr Baillon's remarkable observations under the notice of the Society. By his labours, the morphology of these remarkable plants is now securely based upon the surest of all foundations, viz., that obtained from observation of the different phases of development.

I have myself been much interested in this subject, having had my attention drawn to it by the consideration of some abnormal conditions in the cones of the spruce fir. At our July meeting last year (1860) I exhibited and commented upon some "bisexual cones" which seemed to illustrate certain of the relations between the male and female cones in these plants. I then attempted to show that the *stamens* of the male cone are the parts homologous to the *bracts* of the female; that, in fact, the stamens and bracts are the leaves belonging to the main axis of the male and female cones respectively. At the same time I insisted that, as the male cone consists of a *single axis* with its leaves, it must therefore be considered as a *single flower*—in opposition to a view taken by Dr Lindley in his "Vegetable Kingdom," where he holds, that the male cone consists "of a number of monandrous naked male flowers, collected about a common axis," and that the "anther is formed of a partially converted scale, analogous to the indurated carpellary scale of the females." I gave my reasons for inclining to believe with Schleiden that the "scales" of our ordinary cones are flattened shoots, and attempted to refute Dr Lindley's arguments in favour of the carpellary hypothesis. In conclusion, I started some doubts as to the correctness of the generally received opinion upon the nature of the so-called ovule, founded upon what was known of the development of the innermost "seed-coat" in *Gnetum*, and also from some more theoretical considerations as to the signification of the "corpuscula" of the coniferous "ovule." I was unable, however, to solve these doubts.

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* Read before the Botanical Society of Edinburgh, 10th January 1861.

It was not until some weeks after the publication of my observations that I saw Dr Baillon's paper, which had been published a month previously to mine,* and I was very much gratified to find what I had been occupied with clearly elucidated and established upon a satisfactory foundation.

It is unnecessary for me to give any *resumé* of Dr Baillon's observations, as he has himself given a very clear recapitulation of the main results. He has taken exception to Dr Lindley's arguments relative to the nature of the cone scale, on the same grounds, and, I had almost said, in the same words, as myself.

The relation of the stamens to the pistils in *Pinus* and its allies is very remarkable. The monœcious condition in these plants is neither brought about by a mere suppression of parts in homologous or corresponding male and female flowers, as in many monœcious and diœcious plants; nor by a mere replacement, as it were, of pistil by stamens, and *vice versâ*, as in the male and female flowers of *Salix;* but by the following arrangement:—The male and female cones are corresponding or homologous shoots. The stamens are developed upon the main axis of the male cone; the pistils, on the other hand, are developed, not upon the main axis of the cone, but upon *tertiary* shoots, the "scales" being the secondary shoots axillary to the leaves of the main axis. In the male cone, the secondary and tertiary shoots may be considered as *suppressed;* while, in the female, the stamens are *replaced* by bracts.

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* Dr Baillon's paper was published in September, mine in October, 1860.

I have brought with me, for your inspection, a very beautiful example of the combination of the female inflorescence with the male flower in *Abies nigra.* I alluded to this in a note which I appended to my observations on the "bisexual cones," of *Abies excelsa.* On the lower two-thirds of this cone, the greater number of the bracts are replaced by stamens, which carry the scales or shoots of the female inflorescence in their axils. This curious combination cannot, I think, fail to strike any one reflecting on it with astonishment.

I would now make a few remarks upon the constitution of certain abietineous cones, viz., those of *Cunninghamia, Araucaria, Dammara,* and their allies.

Richard held, that in *Cunninghamia,* both bract and scale are present; in *Dammara,* the scale only; while he seems to have been uncertain as to the signification of the structure in *Araucaria.*[*]

Endlicher considers the cones in all three genera as destitute of bracts. What Richard considered to be the scale in *Cunninghamia,* he terms a transverse torus.[†]

Schleiden holds the "scales" in *Araucaria* and *Dammara* (*Agathis*) to be unprovided with bracts.[‡]

Lindley also seems to coincide with Endlicher and Schleiden in viewing the cones of *Araucaria* as bractless, since he appeals to the leaf-like "scales" of these cones in proof of the carpellary nature of the scales in our ordinary Conifers.[§]

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[*] "*Amentum ovatum; squamis . . . . introrsum unifloris (squamula postica bracteali? veluti destitutis.*") Richard, *Mém sur les Conifères,* &c., p. 87.

[†] *Gemmulæ . . . . toro transverso insertæ.* Endlicher, *Synopsis Coniferarum,* p. 192.

[‡] Schleiden's *Principles,* Lankester's translation, p. 383.

[§] Lindley, *Vegetable Kingdom,* p. 227.

After careful consideration, I have been led to the firm conviction, that in the cones of all these genera the bracts are well developed, while the true scales are more or less reduced in size, or altogether incorporated with the bracts.

In *Cunninghamia*, the bracts and scales are quite distinguishable, as was pointed out by Richard.*

It is hardly necessary here to enter at length in defence of Richard's opinion; the *onus*, in this instance, certainly lies upon those who would refute it. Endlicher describes the structure in *Cunninghamia* and *Arthrotaxis* thus:—"*Squamæ gemmuliferæ plurimæ, ebracteatæ, basi unguiculata insertæ, imbricatæ, supra unguem toro transverso incrassatæ.*"† It is evident, however, that an almost similar description might have been applied to the *bracts* in *Abies pectinata*, since here, as in *Cunninghamia*, there is an *unguis* or claw common to the bract and scale which are united for some distance from their base (compare figs. 1 and 2). In fact, the union, to a greater or less extent, between the base of the bract and that of the scale seems to be of very frequent, if not of universal occurrence in *Abies*, *Pinus*, and their allies.

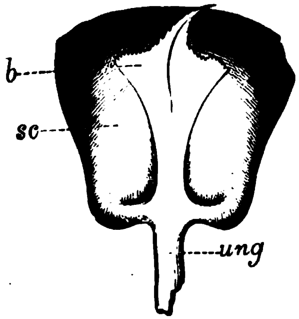

Fig. 1.

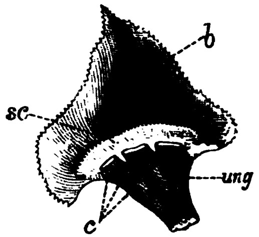

Fig. 2.

Fig. 1. Bract and scale from cone of *Abies pectinata* (Silver Fir); outer surface. Female flowers ("ovules") have been removed. *b*, Bract; *sc*, scale; *ung*, claw common to both bract and scale.

Fig. 2. Bract and scale from *Cunninghamia sinensis*; inner surface. *b*, *sc*, and *ung*, as in fig. 1; *c*, points whence the female flowers have been removed. (After Richard.)

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* Richard, *Mémoires*, p. 81.

† Endlicher, *l. c.* pp. 192, 194.

If a cone of *Araucaria* be examined, it is manifest that the apparent scales of the cone are *serially* homologous with the leaves of the shoot which the cone terminates. As Dr Lindley has pointed out (*Veget. Kingdom*, p. 227), these scales "have actually the same structure as the ordinary leaves;" there is, indeed, a gradual transition from the one to the other. Now, under the ordinary view, the difficulty at once arises; since the "scales" of *Araucaria* are thus the leaves of the main axis of the cone, how can they correspond to the scales of an *Abies*, which (whatever view be taken of their nature), are certainly not the leaves of the main axis of its cone?

This difficulty may, I think, be removed, if we direct our attention to the small scale-like body (figs. 3 and 4, *sc*), situated

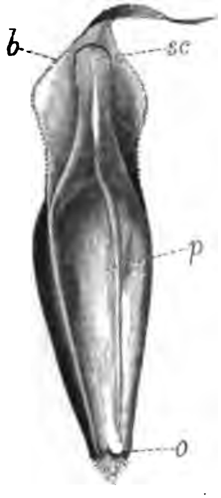

Fig. 3.

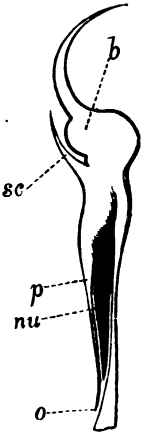

Fig. 4.

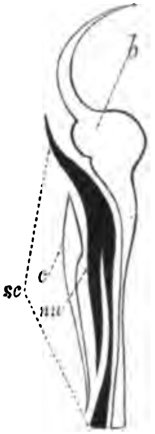

Fig. 5.

Fig. 3. Bract and scale from *Araucaria imbricata.* *b*, Bract; *sc*, scale; *p*, pistil; *o*, orifice of pistil. (After Richard.)

Fig. 4. Longitudinal section in the mesial plane of bract and scale from a young cone of *Araucaria* (*Eutassa*) *excelsa*: *b*, *sc*, *p*, and *o* as in fig. 3; *nu*, "nucleus." From a dried specimen remoistened.

Fig. 5. Diagram showing the supposed constitution of the cone-scale in *Araucaria.* The different parts are represented as detached from one another. The shaded portion represents the axial structure. *b*, Bract; *sc*, axillary scale, *nu*, "nucleus" (the termination of the axis of the scale?); *c*, carpel.[*]

[*] The covering of the "nucleus" in Araucaria is here provisionally termed a carpel. It may, however, be open to question whether this pistil (the so-

near the apex of the apparent-scale in most of the species of *Araucaria.* This "squamula" is most distinct in those Araucarias termed *Eutassa.* It is very small in *A. imbricata,* and is absent in *A. brasiliensis.*[*] It is generally regarded as a process or appendage of the "ovule."[†] This small scale-like body must, I conceive, be regarded as the representative of the cone-scale of an *Abies.* We are prepared for its small size, when we consider the relatively small size of the scale in *Cunninghamia* when compared with the scales of *Abies* or *Pinus,* and for its extensive union with the bract by what occurs in *Cunninghamia,* or *Abies pectinata.* If this supposition be adopted, the structures in the cone of *Araucaria* become at once intelligible, and capable of strict comparison with those in our ordinary cones. What have been termed scales in *Araucaria* should now be considered as bracts, to which the scales proper are extensively adherent.

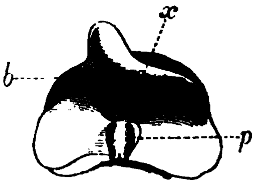

Fig. 6.

Bract (cone-scale) of *Dammara australis.* *b,* Bract; *p,* unequally winged pistil; *x,* point of attachment of the pistil.

In *Dammara* the cone is provided with "scales" so-called, each of which gives origin, about or a little below the centre of its inner surface, to a solitary unequally-winged female flower ("ovule"). These scales have, so far as I am aware, been universally considered as true scales destitute of bracts. This, I believe, is exactly the reverse of the truth, and on the following grounds:—

1st, These so-called scales are not placed in leaf-axils.

2d, In the allied genus *Araucaria* we have the true scale very much reduced in size. Its free portion actually dis-

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called adnate or adherent ovule) may not really be of the nature of an inferior germen; in which case the "carpel" would necessarily be considered as consisting for the most part, if not altogether, of an axial structure. This point, however, can only be determined by examination of the development.

[*] I have not had the opportunity of examining the cones of *Araucaria brasiliensis.* Through the kindness of Professor Balfour, I have examined a cone of *A.* (*Eutassa*) *excelsa,* from a scale of which I made the section represented in fig. 4. The transition between the leaves of the shoot and the so-called scales (bracts) of the cone is particularly well seen in young specimens.

[†] See Richard, *Mémoires,* &c., p. 87; also, Endlicher, "Synop. Conif.," p. 184.

appears in *Araucaria brasiliensis*, in which we have a condition almost exactly similar to what, I believe, exists in *Dammara*. If my supposition regarding *Araucaria* be correct, it cannot be imagined that *Dammara*, otherwise so nearly related, should differ so widely from *Araucaria* and the ordinary Abietineæ as it would necessarily do if the generally received opinion were correct. Thus, I conceive, that in *Dammara* we have, as in *Araucaria brasiliensis*, bracts with which the axillary scales (so to speak) are completely incorporated. On cutting the cone-scale of *Dammara* longitudinally, the female flower (" ovule") is seen to be supplied with a vascular bundle, which runs up from the base of the " scale," curving at its upper end to enter the inverted pistil. This vascular bundle is quite distinct from the mesial vascular bundle of the so-called scale, running parallel to it, and like it, in the mesial plane. This circumstance, although it does not of itself necessarily prove anything, yet tends to confirm the idea that the pistil of *Dammara* does not *originate* from the scale so called, but is only *connate* with it by its peduncle.

I feel satisfied that no one, who will without prejudice reflect upon the matters which I have just now been discussing, can fail to be convinced that the above is the only possible method by which the structures in *Araucaria* and *Dammara* can be reduced to a common type with our ordinary Coniferæ.*

There is one other question of interest in relation to the genera *Araucaria* and *Dammara* to which I may allude, although it is one which can only be determined with certainty by an appeal to their developmental history, viz., are their solitary pistils (so-called ovules) developed on tertiary axes, as in *Abies*, &c ; or (what is probably the case) are they developed upon the extremity of the secondary axes—the " scales," or axillary shoots of the bracts? Upon this latter supposition we probably have in *Cunninghamia* a transition form between *Abies*, *Pinus*, &c., and *Araucaria*, with its allies. In *Cunninghamia* it is probable that, in addition to the two lateral flowers of *Abies*, &c., a terminal flower is developed at the extremity of the axis of the scale. In *Araucaria*, &c., the two lateral flowers are not developed, leaving only the terminal one.

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* It may be observed that the above reasoning is not necessarily dependent upon any considerations as to the nature of the so-called ovules (pistils of Baillon), or of the *squamæ fructiferæ*.

If there is any truth in this idea, it might be predicated, that of the three female flowers on the scale in *Cunninghamia*, the central one would be found to be developed first, the two lateral subsequently, a state of things resembling the centrifugal development of the female flowers in *Cupressus*, so clearly detailed by Dr Baillon.

The union of the bract with the axis of inflorescence in *Cunninghamia*, and with the peduncle in *Araucaria* and *Dammara*, is perfectly comparable to the connation of the axis of inflorescence with the leaf from whose axil it originates, in *Tilia* and *Helwingia*.

On the Acrid Fluid of the Toad (Bufo vulgaris). By JOHN DAVY, M.D., F.R.S. Lond. and Edin., &c.*

The fluid, the secretion of the cutaneous glandular structure of the toad, is a subject on which many different opinions have been entertained. The popular notion, handed down from a remote period, has always been that it is a poison. That notion some years ago was held to be a mere vulgar prejudice, and was pointed out as altogether an error, by so high an authority as Baron Cuvier.† In 1826 my attention was given to it, and the result of the inquiry was, that the fluid is simply acrid, an irritant, offensive as such, but not a poison.‡ Subsequently, it was examined by two French physiologists, MM. Gratiolet and S. Cloez. The conclusion they arrived at was, that the secretion is a powerful poison, in some instances occasioning almost immediate death.§ Later still, Mr George Rainey engaged in the inquiry, and his results,* it would appear, are opposed to those of MM. Gratiolet and Cloez, agreeing perfectly with those I had before obtained.

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* Read before the Royal Society of Edinburgh, 18th March 1861.

† *Le Règne Animal*, ii. p. 94.

‡ Phil. Trans. for 1826.

§ A notice of these experiments is to be found in the "Spectator" newspaper of 28th August 1852.

Recently, viz. in August last, I made further trials of the fluid. The animals experimented on were a kitten, two days old, a fowl, and a slug (*Limax ater*). The toad, from which the fluid was collected was of ordinary size. It had been in confinement three days.† The fluid was obtained by scraping the glandular surface secreting it with a knife. In each trial it was applied to a small wound in the cutis; and not merely applied by simple contact, but inserted and rubbed in. The animals were watched for twenty-four hours. The results were entirely negative. Neither the kitten, the fowl, nor the slug appeared to suffer in the least. The kitten did not refuse milk, although the part chosen for the experiment was the nose; nor had the fowl its appetite impaired;—its head was the part selected for the trial.

These results agreeing with my former, and with those obtained by Mr Rainey, I shall not attempt to reconcile with those, so opposite, of the French physiologists.

I do not think that any material difference of quality of the fluid could have arisen from difference of climate or of season. My first experiments were made in the Ionian Islands; and the viscid fluid used—at the time copiously secreted—was acrid and irritant. The fluid last employed had the same bitter taste, and, applied to the tongue, excited the same kind of sensation as before experienced—an acrid one, but not severe, though of some hours' duration.

I have stated, in my early account of the fluid, that it was neither alkaline nor acid, it having had no effect in changing the colour of litmus or turmeric paper. MM. Gratiolet and Cloez say that they found it alkaline. The fluid which I have recently examined has also been neutral, similarly tested. They mention having kept it twelve months without its losing its activity. If kept in a damp place, might not its alkaline reaction have been owing to a little ammonia developed? A small portion of the acrid matter collected from a toad in Barbadoes, in 1847, now, in 1860, shows a decided acid reaction. It was melted, when collected, at a low temperature, just sufficient for its fusion, after being dried. It has been kept since in a dry place, and it seems unaltered in its properties. It is bitter, and, judging from its effect when applied to the lip, it is even more acrid than the fresh fluid of our English toad. Moreover, mixed with water, its character under the microscope is similar—minutely granular; and I have not found it different in its effect. When applied to a fowl in the manner before described, the result was equally negative.

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* Quarterly Jour. of Micro. Science, No. 12, for July 1855.

† During that time it had voided a good deal of excrement. The fœcal part consisted chiefly of the wings of insects; the liquid, the urinary part, contained a little urea.

The toad of Barbadoes, it may be mentioned, if not identical in species, has a great resemblance to our toad. It differs chiefly in being of a lighter colour, of a somewhat more slender make, and more active in its habits. Incidentally I may remark, that though introduced from Dominica only a few years ago, it has now spread all over the island; also, that it is considered poisonous there by the natives. It is said that dogs that had eaten it became mad. Perhaps the acrid fluid made them wild, and their disturbed state was called madness. It is also asserted, that now the dogs have gained experience of its effects, they leave the toads unmolested, carefully avoiding them. For the accuracy of these reports, of course I cannot vouch. It would be an interesting circumstance, were it sufficiently well authenticated, that the experience of one dog, or generation of dogs, had been transmitted, and the acquired aversion become hereditary, as is known to be the case with some of the habits of brute animals.

I have stated that the microscopical character of the acrid matter of the toad of Barbadoes is similar to that of the toad of this country. Both consist of a fluid in which are suspended an infinity of granules of an extremely minute size, so much so as not to be discernible under a quarter of an inch object-glass, and yet, when seen with the one-eighth inch glass, though excessively small, they are well defined. In one instance* I estimated their diameter to be about $\frac{1}{20000}$th of an inch.

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* The fluid mentioned above was from a male in the month of April, in the

In relation to the use of the acrid fluid, I have before expressed the opinion, that as it is so offensive to the taste, and irritating, it is well adapted to secure the sluggish and otherwise defenceless animal from being devoured. I may mention, in confirmation of this view, that the secretion seems to be in some measure under the will of the animal. When I have seized one of its limbs with a forceps, or pinched its skin, the acrid matter has been immediately poured out, and not only in the exact spot where the pressure was made, but also from the adjoining surface, and so conspicuously that there could be no doubt about it.

If so protected, a question may arise, how is it that this animal is not more abundant? One reason may be, that the very young toad, after its final metamorphosis from the tadpole state, is, I believe, in a manner defenceless. The expression of belief is used, the conclusion being founded on the examination of a single specimen, and consequently requiring confirmation. Another circumstance may be, the little power possessed by the toad of resisting cold. I have found a temperature two or three degrees below the freezing point fatal to it; for instance, when it has been exposed, unprotected by any covering, to the open air for a few hours during a frosty night towards the end of autumn. Now, as the toad in the early winter, or shortly before its setting in, seeks its hybernating abode, and there becomes torpid, should the season be unusually severe, not having the power of quitting its selected spot, it may be frozen and die. The rapid manner in which the toad has multiplied and spread in Barbadoes, and the greater abundance of, and the larger size attained by this animal in a southern climate, such as that of the Ionian Islands, seem favourable to this conclusion.

neighbourhood of Edinburgh, caught when on the back of a female *in coitu*. It may be worthy of remark, if a single observation may be trusted, that the larger female differs from the smaller male in having her skin apparently destitute of the acrid secretion, at least during the breeding season. In the male, at the same time, it was unusually abundant. The latter, at this season, perhaps, may afford sufficient protection to the former, whilst the ova are passing from the oviduct; and in her, the growth of the ova may determine from the skin, for a time, and render the glandular structure inactive.

On the Geology of the Railway from Worcester to Hereford. By the Rev. W. S. Symonds, F.G.S., President of the Malvern Nat. Hist. Field Club. (Plate VI.)

It is a difficult mental process for us, who live in these days of progress—of the electric telegraph, the steam-engine, and a thousand sciences—to imagine the human history of less than two hundred years ago.

"We must not forget that the country of which we *read* was a very different country from that in which we *live*," writes Lord Macaulay, in his description of England at the time when the sceptre passed away from Charles the Second to the tyrant James. "Could the England of 1685 be by some magical process set before our eyes, we should not know one landscape in a hundred, or one building in ten thousand. Everything has been changed but the great features of nature, and a few massive and durable works of art. We might find out Snowdon and Windermere, the Cheddar Cliffs and Beachy Head. We might find out here or there a Norman minster, or a castle which witnessed the wars of the Roses; but with such rare exceptions everything would be strange to us. Many thousands of square miles which are now rich corn-land and meadow, intersected by green hedgerows and dotted with villages and pleasant country-seats, would appear as moors overgrown with furze, or fens abandoned to wild ducks." In those days the Great Bustard (*Otis tarda*), now extinct, coursed in troops of fifty or sixty over the plains of Wiltshire and Norfolk; the red deer was common in Gloucestershire and Worcestershire; and the wild bull wandered in forests the site of which is now traversed by the steam-engine and the plough. Still less is it possible to imagine the England of 2000 years ago, when the ancient Briton wandered a half-naked savage, or the Druid performed his sacrificial rites. Judging from the sculptures and relics of the earliest historic times, the races of men were as distinguishable then as now, were then associated with particular climates and countries. We have no data which reveal to us the changes which have passed over the form of the typical human race, and produced characters so distinguishable as those of the Negro, Caucasian, or Mongolian. The animals that were contemporary with the earliest *historic* human beings were identical with the forms that now surround us, with the exception of a few which have been destroyed by man's agency, and others which have been removed from their original habitations. The races of domestic cattle are modified to a considerable extent; but there were modified varieties when the records of ancient Egypt were sculptured, and the wild animals are unchanged.

Alluvia of the Historic Period.—The first steps the traveller takes on the line of the Worcester and Hereford railway are steps immensely backward as regards the series of changes they imply, and as measured by years. I shall refer to these just now; but as the different geological changes are very important to enable us to fully understand the history, I shall first call your attention briefly to physical changes which have been brought about in this neighbourhood since the time when man is believed to have existed.

I have reason to believe that the valley of the Severn, the banks of the river, and the river itself, possess much the same natural features between Worcester and Gloucester as in the days of the Druid, or when Caractacus lighted his watch-fires on the summit of the Herefordshire Beacon.

I had the pleasure of presenting that distinguished geologist, Sir Charles Lyell, with a nearly perfect Roman jar, which was obtained by Mr Williams, the contractor of the Tewkesbury Severn Navigation Dock Works, near the banks of the river Severn, and which was covered by a few feet only of the silt strewed over the meadows by the autumnal or winter floods. A funeral urn, containing ashes and portions of human bones, also Roman, was obtained by Mr Strickland of Apperley Court, near Tewkesbury, from his brick-pits on the banks of the Severn, near the Haw Bridge. The Roman jar was nearly perfect when discovered, so was not drifted for any long distance by a river stream. The sepulchral urn was, I have no doubt, buried where it was discovered on the banks of the Severn, and had since been covered by several feet of Severn silt, the produce of the floods of well-nigh 2000 years. In some instances it is evident that rivers have shifted their course and changed their channel; and near the Haw Bridge we can see that formerly the Severn river flowed nearer to the Apperley banks than it does at present, but it was at a period anterior to the times of our British or Roman predecessors.

Period of the Severn and Avon Lakes.—Leaving the river and river banks, the flat and verdant meadows on either side have a geological history belonging to them worthy of our attention.

There was a time, long antecedent to the days of the Romans, when both the Severn and the Avon flowed, as the river Shannon does now, through a chain of various sized lakes. The race-courses of Worcester, Upton, Tewkesbury, and Gloucester, indeed all the rich Severn meadows, were formerly *lakes* forty feet deep, into which the river poured its sediment for ages, and which became gradually silted up until they were first marshes, and lastly rich pastures for flocks.

During the cuttings near Tewkesbury, for the Severn navigation docks, excavated to a depth of forty feet towards the bottom of the lake which once rolled its waters above the race-course, a very perfect antler of the red-deer (*Cervus elephas*) was found at the depth of 37 *feet of detritus*, and was presented to me by Mr Williams. No geologist worthy of the name can examine these old lake-beds without perceiving that it would require many thousands of years for a river like the Severn to accumulate the sediment which has filled their beds.

The animals that roamed on the banks left their bones and skeletons in the sediments of the bygone lakes of Worcestershire. The relics of gigantic extinct quadrupeds are abundant in the lacustrine and river sediments of the vale of Worcester. Until I had closely examined the sites from which Mr Hugh Strickland, the Rev. William Parker, and other collectors, gathered the numerous remains of these animals in the Worcester Museum, and the Strickland collection at Jardine Hall and Apperley Court, I had no idea that such an abundance of mammalian remains could, excepting in caves, have been heaped together in such limited space. From one small brick-pit, at Bricklehampton on the Avon, and which brick-pit was probably a bank of mud on the shore of one of the Avon lakes. Mr Hugh Strickland obtained the remains of the rhinoceros, hippopotamus, the cave hyæna (*Hyæna spelæa*), several kinds of deer, also of the long-fronted bull (*Bos longifrons*), and the still more ancient *Bos primigenius*. It is necessary here to warn the explorer of the Bricklehampton and Cropthorne districts, that there are *two drifts* of very different age, both yielding mammalian remains in the same parishes

The old lake drifts are close upon the borders of the Avon, and lie about thirty feet above the river. You must inquire for the old *brick*-pits, not *gravel*-pits. The Cropthorne and Bricklehampton gravel deposits belong to the glacial drift, occupy higher ground, as around Avon Bank, and Comberton, and furnish the remains of the mammoth (*E. primigenius*), and the extinct rhinoceros (*R. tichorinos*), with *marine* shells, whereas the lacustrine drifts are full of *fresh-water* shells now living in the river Avon; the most abundant species being Cyclas, Lymnea, and Unio. No one accustomed to the fossils will confound a Cropthorne brick-pit specimen with those of the marine drift through which the valley has been cut. The first are of a blueish grey, and the latter of a yellowish tinge.

I said that the long-haired elephant or mammoth (*E. primigenius*), and the long-haired rhinoceros (*R. tichorinus*), have been found in the marine drift. The fine specimens from Himbleton, in the Worcester Museum, are said to have been found in lacustrine gravel, associated with fresh-water shells. As far as my own experience extends I have not been able to identify a single specimen of the remains of the mammoth with the lacustrine and fluviatile drift of Worcestershire, with the exception of some worn portions of teeth, which may have been drifted from older deposits. All the mammoth remains in Mr Strickland's collection are from the marine gravels; as are those presented by Mr William Parker to this museum. From the interior of some of the bones Mrs Strickland has extracted remains of marine shells. Nevertheless, we cannot doubt that hyænas, hippopotamuses, rhinoceroses, and extinct deer and oxen, frequented the shores of our ancient lakes. It is also possible that man was an inhabitant of the British islands during the lake period.

The human skeleton found at Defford* when laying the foundation of the railway-bridge over the Avon was covered over by eighteen feet of lacustrine silt, and, I believe, was deposited in that silt before the bed of the ancient waters was filled up.

The skeleton found at the bottom of a peat-bog near Mickleton tunnel, and which called forth so much objurgation, was probably imbedded about this period. Elevated beds of silt and gravel, in the valley of the Clyde, contain rude canoes hollowed out of trunks of oak by stone hatchets; canoes formed of oaks of great age and large dimensions, proving that the climate differed little from that which exists at present. In some of our British valleys we find canoes, hollowed out by fire or stone axes and chisels, covered by twenty feet or more of sediment; while—a significant fact when reflecting on our Severn and Avon valleys, in the alluvial sediments of the River Aire—there lie nearly complete skeletons of the extinct *Hippopotamus major*, with jaws and horns of deer, and petrified nuts, associated with *buried canoes*, covered by twenty feet of detritus.† When Mr Hugh Strickland, some years ago, mentioned, in a lecture delivered before the Natural History Society of Worcester, the number of relics of large quadrupeds that he had collected from the Bricklehampton drifts, it was said that he had fallen in with an ancient ford where a caravan of wild beasts had been upset and drowned.

Marine Drift.—I shall not, on this occasion, enter at any length on the difficult and obscure history of at least two marine drifts, which may be studied in the vale of Worcester. It will be sufficient to call attention to the beds of detritus through which the Worcester and Hereford Railway passes. An examination of the gravel-pits, at higher levels than the old lake shores in this neighbourhood, will explain satisfactorily the determination arrived at, many years since, by Sir R. I. Murchison—viz., that the vale of the Severn was a strait of the sea ages before the lake and river periods, and that the valleys of the Severn and Avon were gradually scooped out by marine currents and waves, during the long epoch when the present land, on which now stand populous towns and cities, was being slowly elevated.

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* See Strickland on Birmingham and Gloucester Railway.

† Professor Phillip's "Life on the Earth."

The geologist who would realise this history in his mind's eye must stand on the summit of the Malverns, and imagine a broad sea frith or strait washing the base of the hills below him, and covering with sea-waves every hill that rises in the valley between the Malverns and Cotteswoldes, save the summits of Bredon, Dumbleton, Churchdown, and Robinswood Hills, which were low islands in those Severn Straits.

Hartlebury Common is a good locality for studying the elements of the old sea-bed of the Northern Drift, and is easy of access. My friend Mr Roberts of the Geological Society of London, and a Worcestershire geologist, gives a graphic description of the wandering pebbles in the gravel-heaps of Hartlebury Common in his "Rocks of Worcestershire."

I am very much inclined to rank the gravel at Henwick and St Johns, through which the railway passes, as a *marine*, and not as a lake or river drift; and I look upon this drift as about the latest evidence we possess of the existence of the marine conditions of the vale of Worcester.

The period of the Northern Drift was that of the Glacial period, a well-marked period, during which, writes Mr Darwin, "we have evidence of almost every conceivable kind, organic and inorganic, that within a very recent geological period central Europe and North America suffered under an arctic climate. "The ruins of a house burnt by fire do not tell their tale more plainly than do the mountains of Scotland and Wales, with their scored flanks, polished surfaces, and perched boulders, of the icy streams with which their valleys were lately filled." Unnumbered ages have rolled by since that period; but what is time to Him who holds eternity within His grasp, and to whom myriads of years are but as a moment in the records of bygone epochs?

The view from the summit of the Malverns, during the period of the "Northern Drift," must have been very different from the present. Not only did a sea strait stretch far away over the vales of Worcestershire and Gloucestershire, but the eye must have rested, on the western or Herefordshire side, on many an inland frith and narrow strait. The glacial drift lies in the valley of Cradley, between Malvern and Ledbury, and again in the valley of Clincher's Mill, south of Eastnor Castle; while, where now we see only cultivated fields, pastures, and orchards, the valleys of the Wye, Lugg, and Frome, must have gleamed with waves.

Among the inhabitants that frequented the shores of the icy straits and friths, and lived upon the land of Malvern, Herefordshire, and the separated Cotteswoldes, were the long-haired, glacial, elephant (the mammoth), the long-haired rhinoceros, and the extinct ox (*Bos primigenius*).

I have to thank my friend Mr Ballard for rescuing some interesting remains of these great extinct quadrupeds from destruction. I have not been able to trace the former existence of glaciers along the line of the Malverns; but at the southern extremity, from Clincher's Mill to Haffield, we have evidence of the effect of coast ice extending into a small bay between the Malverns and Ledbury Hills, and of the deposition of great masses of angular and unworn blocks of rock on the melting of the ice as it floated out to sea.

While the vale of Worcestershire was a sea-bed, the Malverns could have appeared but as low hills, probably not elevated more than 600 or 700 feet above the surface of the waves, while the Cotteswoldes opposite must have constituted a long, low shore.

During the glacial epoch it was, as Professor Forbes believed, that Great Britain possesed a flora of the Arctic and Alpine species of plants, so assiduously sought for by the botanist, and which are enabled to maintain their footing on our higher mountains still, according to the rules of climatal influence, through the elevation of the tracts which they inhabit.

We cannot doubt that when the long-haired elephant and rhinoceros roamed over the Malverns, and when boreal shells lived in the seas, that the plants of the Malverns partook of the arctic type, that the oaks used for canoes during the lake period could not have existed under those climatal influences of the British isles, while such plants as the herbaceous willow (*Salix herbacea*, and the Alpine gentian (*Gentiani nivalis*), the snow Veronica and Saxifrage (*Veronica alpina*, and *Saxifraga nivalis*), with the moss Campion (*Silene acanlis*) (plants which have, countless ages ago, deserted our Worcestershire hills, and are still found flourishing in the cold regions of our most lofty mountains), flourished on the Malverns.

Having taken great pains to investigate the evidence advanced by our best authorities upon so important a subject, I can no longer entertain a doubt that flint implements, the work of ancient men, are found in beds of drift, which are older than even the long ago period of our Severn lakes. These human implements are associated with the bones of great extinct quadrupeds, which lived during the glacial period. The drifts both of France and Great Britain, which contain this strange assortment of prehistoric human relics and bones of extinct animals, cannot be set down as of later date than of a period intermediate with the Severn lake period and the latter part of the glacial epoch. The drift of the Somme, near Amiens, containing stone implements, occupies a similar position above the River Somme, flowing 100 feet below in a scooped-out valley, as our marine drift, with the remains of the long-haired elephant, occupies above the site of the Severn its scooped-out valley, and the filled-up lakes. Remains of the mammoth (*E. primigenius*), and of the *Rhinoceros tichorinus*, were found in clay-beds overlying the Keuper marls, near the station of Malvern Wells, and between the station at Malvern Link and Worcester.

New Red Sandstone.—The geological starting-point for the railway traveller at the Shrub Hill station, Worcester, is upon the marls of the Triassic or New Red Sandstone period. The cuttings near the station expose sections of the Keuper marls; and a short walk from Worcester to Crowle will take the geologist over a series of ridges, capped by drift, to the Lower Lias of Crowle, which overlies the upper Keuper rocks of the district.

The railway to Hereford, then, starts on rocks of the upper Trias, or New Red Sandstone, overlaid by beds of marine drift. After passing the Forgate Street station, we cross the silted up lake of Worcester race-course and the River Severn. Just beyond St John's station, we meet with beds of gravel and sand, relics of that period when the valley of the Severn was an arm of the sea, and which overlie the Keuper marls. The Keuper marls are seen everywhere in the railway cuttings until we reach the mouth of the tunnel at Malvern Wells. The tremendous gap the geologist has to deal with in passing from the Pleistocene drifts, and their extinct mammalia, to the rocks of the Trias, is more easily expressed than understood. Geologists group together vast masses of rocks, charged with remains of various animals, into a divisionary classification, which represents vastly longer epochs of time than that understood by the term period, as the Primary (or Palæozoic), Secondary, and Tertiary *epochs*. I have alluded to the enormous lapses of time necessary for the fulfilment of the lake period of this immediate district. We have absolutely no elements of calculation as regards the period known as the Glacial epoch in respect of *time;* still less can we deal with the past eras of the upper, middle, and lower Tertiary systems. What, then, shall we say of the age of our Worcestershire rocks of the upper Triassic period.

The Rocks of the New Red of Worcestershire were accumulated in their ocean-bed during the commencement of the Secondary epoch,—that long epoch during which the rocks of the Liassic, Oolitic, and Cretaceous periods were deposited, with all their relics of successive and extinguished life; periods during which millions of animals were created, lived their allotted span, and died out. The Triassic rocks over which our railway traverses bear somewhat of such proportions to the close of the Secondary epoch—the Chalk period,—as the dawn (Eocene) period of the Tertiary eras bears to the period of existing animals. The geologist, moreover, must bear in mind, as he traverses our Worcestershire Trias, that the upper beds of this series of strata have furnished the palæontologist with the relics of the first known mammalian animal, the Microlestes, or little beast of prey;

while we know that in America "the Brontozoum," a giant among birds, paced over the Triassic shores.

Time! Will the astronomer measure us off, in space, the number of inches that reckon the distance to the nebulæ of the heavens? then may we hope to calculate the course of years that have elapsed since the rocks of the Trias were strewed in the sea-bed which contained the skeleton of the Microlestes, the Rhynchosaur of Worcestershire, or since the sun shone upon the Triassic mud-flats of America, across which there paced birds which left prints of their feet twenty inches in length.

I now enumerate briefly the series of strata and rocks that the railroad passes through between the entrance of the Malvern Tunnel, at the Wells, and the exit of the Ledbury Tunnel at Ledbury, near the turnpike on the Hereford coach road (Plate VI.). We have drift overlying New Red Sandstone on the *east* of the Malvern Hills; Syenite, Diorite, and Greenstone forming the nucleus of the Malverns; Upper Llandovery beds and Woolhope shales in the tunnel; the Woolhope limestone, Wenlock shales, Wenlock limestone, and Lower Ludlow rock, on the *west* side of the hills, followed by some beds of the Old Red series, violently faulted against the Ludlow rock at the extreme west end of the Malvern Tunnel. Then the open railway passes over Upper Ludlow rocks and some lower beds of the Old Red series, here and there covered with drift, until the Lower Ludlow rock is again traversed at the east end of the Ledbury tunnel. This is much faulted and brought up against Upper Ludlow shales and Aymestry rocks. The Wenlock shales and the Wenlock limestone are then traversed; these are much faulted, the Lower Ludlow beds again coming in, followed by Aymestry rock. Upper Ludlow shales, Downton sandstone, and, at the west end of the tunnel, by red and mottled marls, grey shales and grits, purple shales and sandstones, with the Auchenaspis fish-beds, forming the passage-beds into the Old Red Sandstone, as described in my former paper (*Quar. Journ. Geol. Soc.* vol. xvi).

The beds at the entrance to the Malvern Tunnel consist of the upper grey and red marls of the Upper Keuper series of deposits, overlaid by a considerable thickness of debris, derived from the chain of the Malverns above,—an angular, local debris, due to atmospheric agency, and which overlies the northern drift, represented along the line of the Malverns by a strong tenacious clay, containing mammalian bones.*

The tunnel section shows many feet of red and grey marls, passing into Keuper sandstones, much twisted and contorted, proving considerable movement along the axis of the Malverns since the deposition of the Triassic deposits. The lower red marls below the Keuper sandstones rest immediately against the syenite, at the distance of about two hundred yards from the entrance to the tunnel. They dip from the syenite at an angle of 55° to the south-east, and at the point of contact are much crushed and broken; as indeed is the syenite itself for several yards from the point of contact with the lower red marls.†

Syenite and Plutonic Rocks.—I have never passed through the Malvern Tunnel without a feeling of awe, knowing, as I did, that in examining the syenitic nucleus of the hills I was contemplating a record as ancient as aught we know of in our planet's existence, a portion of the crystallised skeleton of this globe, a fragment of the planet's crust, which was consolidated before the deposition of those sedimentary deposits in which we first detect evidence of the existence of living beings.

There is no doubt that the syenitic ridge of the Malverns was formerly a Plutonic mass of lava-like rock in the interior of the planet, and that it constituted a portion of the sea-bed of a primeval ocean, after it had become cooled, crystallised, consolidated, and upheaved to the surface. We have evidence, along the flanks of the Chase-end and Ragged Stone Hills, at the southern extremity of the range, that rocks as old as the Cambrian deposits of the Longmynd and the Lingula flags of Wales were deposited upon the old syenite, when it was as much consolidated as at the present time, for the Cambrian Hollybush sandstone has been seen in contact with the syenite, and this without a sign of metamorphism or alteration by heat. Whether the Hollybush sandstones and the overlying Lingula flags, with their little trilobites, Oleni and Agnostus, their Dictyŏnema and Obolina, were deposited upon a flat syenitic surface, or whether there was then a low submarine ridge running, as now, N.E. and S.W., we cannot say.

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* Since this paper went to press the author has examined a number of teeth of the rhinoceros, and portions of mammoth tusks, found in clay overlaid by local debris at Great Malvern.

† See the author's paper on the "Triassic Rocks in the Vale of Worcester," "Edinburgh New Philosophical Journal," April 1857, p. 257.

We learn from the examination of the southern Malverns that igneous molten matter has been injected into fissures, and passed through crevices in the Hollybush sandstones and Lingula flags, along the flanks of the syenitic ridge, both east and west of the ridge. This fluid matter must have been ejected from below, through fissures in the solid syenite, which probably was then slightly elevated along the line of the axis of the Malverns. Altogether, we may be satisfied that volcanic action and earthquake movements took place along the axis of the Malverns during a period intervening between the deposition of the Lingula flags and the deposition of the Llandovery rocks (Upper Silurian deposits); for those Llandovery deposits, with their included fossils, have been laid down on trap, which had become cold and hardened before the deposition of the overlying sedimentary deposits, for they present no sign of metamorphism within a few yards of the trap.

The great mass of Lower Silurian rocks, described so graphically by Sir R. I. Murchison, in his last edition of Siluria, the Llandeilo and Bala (or Caradoc) group of deposits, many thousand feet thick in Wales, are altogether absent along the flanks of the Malverns. This group of rocks we might expect to find deposited as well as the Lingula flags; but they are wholly absent, their situation being occupied, along the flank of the south Malverns, by a range of trap bosses, which are succeeded by Upper Silurian strata (Llandovery rocks), which at Malvern Wells have been laid down on the syenite itself, and infiltrated into its crevices; the interposed traps, the Lingula flags, and the Hollybush sandstone, being all deficient in the Malvern Tunnel. These rocks must have been denuded by the action of marine currents along the flanks of the *northern* Malverns previous to the deposition of the Llandovery strata. Traversing the Malvern Tunnel (Plate VI.) from east to west, you pass from the red marls of the Trias through syenitic rock, which is succeeded by great masses of graphitic schist, greenstone, chlorite, diorite, and hornblendic rocks, with large lumps of highly crystallised felspar, and these are are again succeeded by syenite, in a crevice of which there appears the vein of Llandovery limestone just alluded to. This wedge of sedimentary rock, which is unaltered, and contains the characteristic fossils, *Orthis calligramma* and *Pentamerus oblongus*, must have been infiltrated into a fissure in the syenite, when washed by the waves of the upper Llandovery period. I shall not easily forget the days of exploration among the rocks of the Malvern tunnel, or the hearty and patient assistance I received from my friends, the Rev. Reginald Hill, Mr Alan Lambert, Captain Peyton, and the Rev. Mr Smith of Tarrington. For some beautiful sketches made in the tunnel, I have to thank Captain Selwyn, R.N.

At about the centre of the tunnel a series of Llandovery limestones, shales, and sandstones, with many fossils, rest almost vertically against a wall of syenite, and these pass conformably into the blue Woolhope shales. Dr Grindrod of Townshend House, Great Malvern, has done wonders for the palæontology of Malvern, and possesses a fine collection of fossils from the tunnel beds.

The details and measurements of these rocks will be found in my paper read before the Geological Society of London, all the measurements having been carefully made by Mr Alan Lambert, one of the engineers of the line. I am very much indebted to Mr Lambert for his valuable assistance in this somewhat arduous undertaking; indeed, without it, the sections and measurements could hardly have been undertaken. The Woolhope shales and limestone are succeeded by the Wenlock shales, and when near the western mouth of the tunnel the Wenlock limestone is quarried. Here occurs a considerable fault. The Wenlock limestone partakes of the fault—the Lower Ludlow beds are thrown down altogether, with the exception of a few yards of crushed clay; the Aymestry rock and Upper Ludlow shales are thrown down vertically, and some sandstones of the Lower Old Red abut against Lower Ludlow and Wenlock beds. The Old Red Sandstone is seen near the western mouth of the Malvern Tunnel. This, therefore, appears the place to say a few words on the marvellous history connected with the physical geology of the western side of the Malverns, and through which our railway passes.

Forgive me if I do not state succinctly, in a few lines, the details involved in a problem which cost Murchison, Phillips, and Strickland, years to work out, and their followers years to comprehend. All that I can do is to mention briefly the leading particulars, which have been gradually arrived at, and are now accepted by all who have studied the problem where the physical geologist works out his proofs—on the mountain side, in the deep glen, in the recesses of brooks, in half filled-up quarries, and entangled woods.

Sir R. Murchison and Professor Phillips, some years since, determined that the syenitic ridge of the Malvern Hills forms a part of a great line of dislocation, extending for at least 120 miles, from Flintshire on the north to Somersetshire on the south—that this line of disturbance forms the eastern boundary of that vast region of elevation which includes the whole of Wales, and that the *principal* movement which caused this elevation took place between the Carboniferous and Triassic epochs, although evidence of later elevatory movements is not wanting.

For unnumbered ages the waves of an ocean rolled over the Plutonic masses of which our hills are probably but the merest fragment; and for unnumbered ages thousands of feet of mineral masses accumulated around and above the Plutonic reef that stretched north-east and south-west across the sea.

We know that the syenite had been consolidated and cooled before even the deposition of the Hollybush sandstone. It probably was slightly elevated along the Malvern axis before the deposition of the Llandovery sandstone; but the longer I study the history of our giants of time, the more I am convinced that this upheaval was slight, and that the whole mass of the Upper Silurians, the Old Red Sandstone, and the Carboniferous deposits, was once accumulated thousands of feet above that Malvern ridge which now stands out so boldly above the plain.

Here I would ask attention to a graphic description, by our lamented friend Mr Hugh Strickland, of the phenomena which occurred after the deposition of the Carboniferous deposits. This description occurs in some unpublished lectures which he delivered at Oxford, and the sketch (fig. 1) represents the accumulation of strata above the syenite at the close of the Carboniferous period.

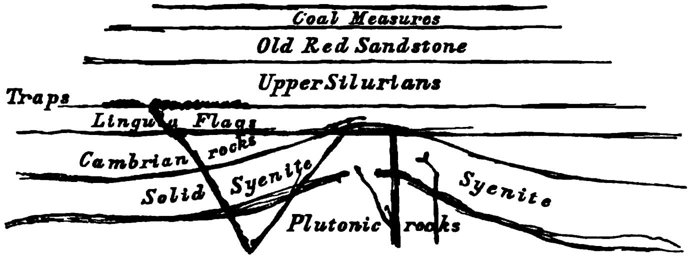

Fig. 1.

" The syenitic axis of Malvern, eight miles long, about half a mile wide, and almost perfectly straight, naturally suggests the idea of a vast dyke of injected trap-rock. But Professor Phillips has successfully shown, from the absence of lateral ramifications of syenite, from the rare and slight indications of metamorphic action, and from other phenomena, that the Plutonic ridge must have been elevated in a solid state. Indeed, the fact that it occurs, not on a line of simple fissure, but on a line of fault, is conclusive of its having been elevated as a solid; for the downcast side being lower by several thousand feet than the upcast, the syenite, if fluid, could not have been raised to its present position, but would have overflowed the downcast side to a great distance.

" Admitting this wall-like mass of syenite to have been forced up from below in a solid state, we at once obtain a clue to the vertical or highly inclined (sometimes reversed) position of the sedimentary strata on the west or upcast side of the Malvern ridge. It appears, then, that the Malvern district, though forming part of a great line of fault, yet exhibits the phenomena of fault under a very complicated aspect. To explain this, I must refer for a moment to a few elementary principles. In the simplest form of a fault, when one portion of a horizontal stratum is elevated by an equally diffused pressure from below, while the other portion remains at rest, the stratum preserves its horizontality up to the very plane of separation (fig. 2).

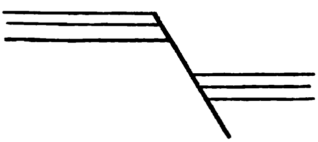

Fig. 2.

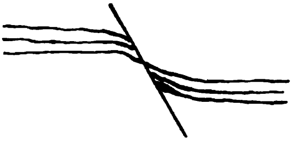

Fig. 3.

Or, more frequently, the friction of the two masses causes the strata to bend slightly *towards* each other on the opposite surface of the fault (fig. 3). Again, if the upward pressure be confined to a *line*, instead of being spread over a *surface*, the strata are thrown in opposite directions, and an anticlinal, with or without a fault, is the result (figs. 4 and 5).

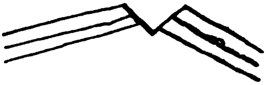

Fig. 4.

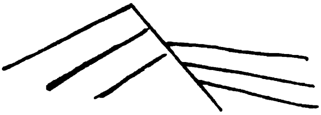

Fig. 5.

"But the Malvern region presents us with a construction of both these kinds of forces, and of both their resulting phenomena. There has been an elevatory force diffused, more or less equally, under a vast area, which has heaved up in a mass the entire region for hundreds of miles to the westward of the Malvern axis. And there has also been a special force applied immediately beneath this axis, which has given an extra amount of elevation to the marginal portion of the upcast area.

"It is this excessive development of motive force *at the very margin* of an elevated region, and in immediate contact with a non-elevated tract, that renders the phenomena of the Malvern Hills peculiarly anomalous. Under ordinary circumstances, when an upward force is applied *locally* above a line, it acts equally on both sides of that line, elevating the strata, as already shown, into an anticlinal position. If, however, the resistance be greater on one side of the axis than on the other, a certain amount of displacement takes place, and the anticlinal arrangement is combined with that of a fault (fig. 6).

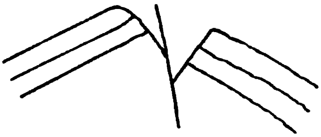

Fig. 6

"The Malvern elevation is probably an extreme and unusually exaggerated instance of the last class of phenomena. If we could strip off the thick mantle of New Red Sandstone which conceals the eastern side of this axis, we should probably find the strata from the Hollybush sandstone up to the Coal Measures more or less upturned at their edges (fig. 7). So vast a force as was required to elevate the syenitic axis could hardly have failed to shatter and twist up the margin of the deposits on its eastern or downcast side, although their amount of stratical resistance was such as to forbid any general elevation of them *en masse*.

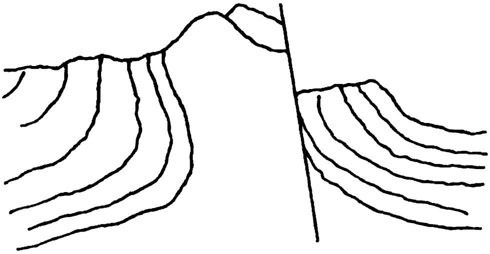

Fig. 7.

"Assuming that the above diagram represents the probable appearance of this region before the deposition of the New Red Sandstone, let us endeavour to trace the mode of action of the forces which produced it. There is evidence that elevatory movements have taken place along the axis of the Malvern chain before, as well as since, that great and transient outburst which dates between the Carboniferous and Triassic epochs. A mass of syenitic rock had been elaborated by igneous agency beneath this tract in very remote geological times. It had become solidified, and had been elevated above the oceanic surface before the Upper Silurian formations were deposited. The sections on the west side of the Malvern Hills show that the mollusca and corals of the Caradoc* (Llandovery) sandstone lived and flourished in immediate contact with the plutonic rock, and that pebbles of the latter were rolled into the sea of that period, and there imbedded in company with the animal remains.—(See *New Geol. Survey*, vol. ii. p. 33). We may therefore suppose that at the period of the Caradoc (Llandovery) sandstone a state of things prevailed such as is here represented (fig. 8).

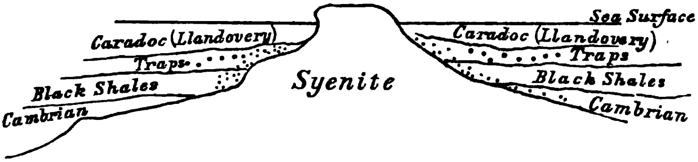

Fig. 8.

"To what extent the Cambrian series, whose thickness in Wales exceeds 20,000 feet, may here have underlaid the Caradoc (Llandovery) sandstone, we have no evidence. All that we know is, that the syenite had been consolidated, cooled, and partially elevated before that portion of the Silusian series known as Caradoc (Llandovery) sandstone was deposited. This most ancient elevation of the syenite was probably small in amount, and was wholly covered up by the formations which succeeded the Caradoc (Llandovery) sandstone, and which contains no fragments of syenitic rocks.

"In order to explain the phenomena which now took place, it may be legitimately assumed, that the floor of solidified syenite on which the sedimentary deposits rested was itself underlaid by igneous rocks in a fluid and active state. Let it be further granted that the present *breadth* of the Malvern syenitic (averaging half a mile) approximately represents the *thickness* of this hypogene stratum of solid plutonic rock.

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* When this paper was penned by Mr Strickland the Llandovery beds of Malvern were believed to be Caradoc rocks, until the mistake was discovered by Professor Sedgwick.

"Such I assume to have been the condition of things when that vast elevatory movement which upheaved the westermost side of our island commenced. It is irrelevant here to inquire whether this general upheaval was effected by the mere expansion caused by increasing temperature, or by the introduction from other quarters of vast masses of fluid matter beneath the elevated area. It will be sufficient to admit that a special volcanic focus existed beneath the syenitic axis of Malvern, and that its energies were called into action simultaneously with the more general movement which elevated the area of Herefordshire and Wales.

"We may now suppose that the elevatory forces beneath the Cambrian region had accumulated so as to overcome the superincumbent weight, while the region to the eastward, either from its greater rigidity, or from the less amount of subjacent force, remained in a quiescent state. A separation would now take place between these two areas; a long and sinuous line of fracture would divide them, and the region where force had overcome resistance would begin to rise higher and higher above the area which remained unmoved. The previous elevatory movement, which has been shown to have existed along the Malvern axis, probably rendered this a weak point in the earth's crust, and caused the line of fracture to coincide with that axis. As soon as one side of this line began to rise, and a fault to be produced, the volcanic forces which had been pent up beneath the syenitic axis would now find, or endeavour to find, a vent. Struggling to escape along the line of fault, they would thrust up the solid syenite above them, raising it above the downcast area, and elevating, overturning, or crumpling up the edges of the Silurian, Devonian, and Carboniferous strata, which rested upon it.

"In the section (fig. 7) in which those portions of the strata which have been removed by denudation, or which lie too deep to be visible, are supplied conjecturally, I have supposed that a vast mass of Silurian, Devonian, and Carboniferous rocks has been upheaved *en masse*, the lower strata near the syenite being more or less fractured, crushed, or contorted. The thickness of the denuded strata may appear enormous, but it is founded on the careful measurements of the Geological Survey, which give more than 5000 feet for the Old Red Sandstone of Herefordshire, and 2500 feet for the incumbent Coal Measures at the nearest point (Dean Forest) where the undulations of the beds have saved them from denudation.

"The volcanic matter which I suppose to have thus forced up the solid strata may itself have never reached the surface. The plutonic axis of Malvern seems only to exhibit its upheaving effects, and to show no signs of fluid ejections contemporaneous with the elevation. It is possible, however, that volcanic matter may have poured out over the downcast area, where it is now concealed by the New Red Sandstone; and the laterally injected dyke of Brockhill, as well as the trappean masses in the black shales on the west of Raggedstone and Midsummer Hills, are not improbably connected with the volcanic forces which thrust up the syenite.

"The detached indications seem to show that the volcanic matter which underlies, and which has elevated this region, is different in mineral character from the more ancient syenite of Malvern, and is probably more allied to greenstone or basalt. The trap-rocks of Wire Forest, north of Apperley, further corroborates this view.

"In tracing to the north or to the south that long line of dislocation of which the Malvern Hills form a part, we find a continuation of analogous phenomena, more or less modified by local circumstances. The Apperley range of hills is, as is so ably shown by Professor Phillips (*Rep. Geol. Surv.*, vol. ii. p. 145), completely analogous to the Malvern district, the chief difference being, that the syenitic axis which upheaved the Silurian rocks is here almost wholly concealed from view, and (with one small exception) is known only by its effects. The Silurian and Old Red Sandstone formations are here, as on the west side of Malvern, *overturned* for a distance of several miles. It is certain that an enormous fault-line runs along the eastern side of all this disturbed and elevated district, and the downcast region on the east has remained relatively rigid and unmoved. In accordance with the well-known law, that the plane of a fault (almost invariably) dips *towards the* *downcast side*, it is evident that the surface of the fault would act as an 'inclined plane,' or wedge, in reference to a vertical uplifting of the strata on the west, and would force them over to a certain distance in a lateral direction (fig. 9). The same natural force would explain the sharp anticlinal curves into which some parts of the Ridge Hill near Apperley are compressed, as shown in 'Rep. Geol. Surv.,' vol. ii. p. 151.

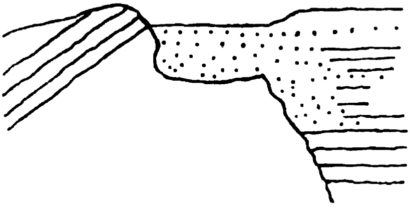

Fig. 9.

"At numerous other points, as we proceed northwards along the eastern limit of the elevated district, or southwards by May Hill to Tortworth, we find indications of the same great line of fault (fig. 10). Sometimes (as at Oswestry and Higley) these faults have affected the Lower New Red Sandstone as well as the Carboniferous rocks, proving that there, at least, the elevatory movement was subsequent to the commencement of the Permian epoch. The great marginal fault seems to have formed a nearly vertical cliff, against which the Upper or Triassic portion of the New Red Sandstone was deposited, as in the Shropshire coalfield at Bewdley, Apperley, Malvern, May Hill, and Pyrton Passages. The Cambrian and Herefordshire area having now become elevated above the eastern region, and the volcanic forces having spent their energy in thrusting up and overturning the syenite and incumbent strata of Malvern, a period of comparative repose ensued. The elevated region had become dry land, while the downcast area remained beneath the sea. The sands and marls of the Triassic series filled up the bed of this sea; while its littoral waves, beating against the syenitic cliffs of Malvern, formed accumulations of conglomerate such as those of Rosemary rock and the Berrow and Woodbury Hills. Not unfrequently the Triassic ocean seems to have wasted away this ancient sea-coast for a

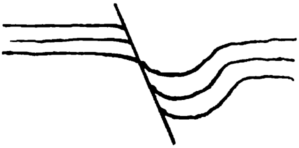

Fig. 10.

considerable distance, so as to have overlaid with detritus the actual line of the fault.

"The Malvern Syenite, from its solidity, resisted this denuding action; but immediately to the west and south of this ridge, the Triassic ocean has excavated considerable bays into the elevated strata, and frequently the great fault line with detritus (fig. 11). The faults which penetrate the New Red Sandstone and the incumbent Lias prove that occasional disturbances still continued to affect this district. The gently-inclined beds of New Red Sandstone at Great Malvern point to a slight renewal of the elevatory force. But these later disturbances were trifling when compared with the forces which were in action at the close of the Carboniferous period. The succeeding ages may be regarded as periods of repose. The Oolitic formations were successively piled upon the Triassic rocks, and may possibly have raised this downcast area to the same level as the upcast portion. The elevated area meanwhile was undergoing a vast amount of denudation. During the long ages of the Triassic and Oolitic systems it was doubtless exposed to atmospheric degradation, and was supplying the adjacent ocean with much of its sedimentary matter, as is ably shown by Professor Ramsay (*Mem. Geol. Surv.*, vol. i. p. 297). The denuding forces, which were so active in the Pliocene period, terminated their vast operations, and gave to this rugged and dislocated area those smooth and undulating outlines which it now presents, and which are further modified by the daily action of the atmosphere."

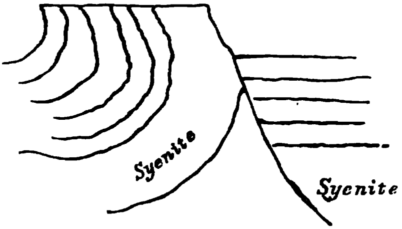

Fig. 11.

This is a summary penned by a master-hand, alas! now cold in death, of some of the phenomena connected with the Malvern syenite and the stratified deposits piled against the western side of the hills, the newest of which is the Old Red Sandstone; as the overlying Carboniferous rocks have been denuded from the Malvern area, though preserved around the forest of Dean.

It is a marvellous lesson in physical geology—a lesson that requires long and patient study among the distant hills of the Welsh coal-field, the mountains and the valleys of Herefordshire, thoroughly to appreciate; nevertheless I would recommend the young geologist to make himself acquainted with certain principles of the geologic histories he will have to deal with before he leaves the rocks of Old Malvern or the first fragment of Old Red Sandstone that rests at the western base of the hills, and is near the exit of the Malvern Tunnel.

The railway passes over these Old Red beds, generally obscured by drift, until we reach the Bartons, the residence of Mrs Peyton, a large brick house on the left hand, and which stands on Upper Ludlow Rock, dips to a synclinal, and rising again into a considerable hill on the right, above Hopend, strikes westwards, in a long ridge, towards Ledbury. The bridge, across which the railway traverses immediately below the Bartons, rests upon a foundation of Lower Old Red faulted at a high angle against the Upper Ludlow rock. My attention was drawn to this interesting fact by my friend Captain Peyton; and on examining the rock, I found it to be no other than the grey fish beds of the passage-rocks between the Upper Silurians and the Old Red, the Auchenaspis beds of Ledbury.

Upper Ludlow shales, overlaid by drift which have afforded remains of the mammoth (*E. primigenius*) and rhinoceros, succeeded by Aymestry rock and Lower Ludlow shales, are seen in the cutting on entering the Ledbury Tunnel. The red clays and shales overlying the Lower Ludlow beds in the cutting are relics of the Old Red, which, now denuded, overlaid the whole of this district probably to as late a period as that of the glacial drift.

We enter the Ledbury Tunnel in the Lower Ludlow shales, and, at a short distance from the entrance, these beds are much faulted and brought up against Upper Ludlow shales and Aymestry rock. This fault has been sketched by Mr Alan Lambert. The Wenlock shales and limestones are then traversed; these also are much faulted, the Wenlock limestone being actually thrown down into a horizontal position, the workmen having to quarry for a considerable distance along the strike of this hard limestone. The Lower Ludlow beds again come in here, in their proper position, followed by the Aymestry limestone with *Pentamerus Knightii*, Upper Ludlow shales, and Downton sandstone, which pass conformably at the western end of the tunnel, though at some distance from the mouth, into red marls, and, at the tunnel mouth, into a gray grit, the passage-beds into the Old Red Sandstone.

In my communication to the Geological Society of London, " On the Passage-Beds from the Upper Silurian rocks into the Lower Old Red Sandstone at Ledbury,"* I expressed my conviction that nowhere in the world is there exhibited such a view of the passage-rocks between the Silurian and Old Red systems as at the entrance to the Ledbury Tunnel on the Worcester and Hereford Railway.

With the aid of my friends, Mr Richards, engineer, and the Messrs Ballard, contractors on the Hereford and Worcester Railway, I worked for hours in the tunnel with the measuring line and clinometer, and we ascertained beyond a doubt that the beds are *perfectly conformable* from the Aymestry limestone in the tunnel to the Old Red beds at the end of the cutting near the turnpike. There is not a sign at Ledbury of the fault which at Ludlow ruins the succession of the passage-rocks, and has led to much misinterpretation respecting the true position of some of the beds.

The fossils from the Upper Silurian rocks of the Ledbury Tunnel are abundant, the principal discovery being that of the *Pentamerus Knightii*, by Henry Brooks, shoemaker, of Ledbury, who has done more than any one in the neighbourhood towards elucidating the palæontology of the Ledbury rocks, by his assiduous collection of the fossils. The Downton bed in the tunnel furnished to the researches of Mr Collingwood of London the characteristic Downton *Lingula*, which has been found also in red and gray beds higher in the series, associated with the larger species of the tilestones of Murchison, *Lingula cornea*.

Both in Ledbury Tunnel and at Hagley, Lugwardine, near Hereford, the yellow Downton beds are seen to pass conformably into a series of red and mottled marls and sandstones, which had furnished a lingula, and, at Ledbury, a portion of the dermal plate of that little cephalaspidean fish, Pteraspis. The Earl of Enniskillen possesses this specimen; and neither his Lordship nor Sir P. Egerton, who examined it at the meeting of the British Association at Glasgow, could distinguish between this plate and that of *Pteraspis Lloydii* of the Lower Old Red.

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* Quart. Journ. Geol. Soc., May 1860.

A band of gray shale, and thin, gray gritty sandstone, now bricked up, just within the entrance to the tunnel, furnished portions of that fine Cephalaspis, *C. Murchisoni*, and a pincer of Pterygotus to the researches of Henry Brooks.

The lover of physical geology may still see the gray fish beds (Auchenaspis grits) at the western entrance of the Ledbury Tunnel; but, alas! we can never restore the picture, once presented to our view, of the gray curtain of rock, charged with the relics of long extinct fish, that stood so boldly out with red shales in the foreground, and purple shales in the background—a monument of gray stone set in a framework of Nature's own manufacture, and sculptured with those hieroglyphics the geologist loves to decipher.

The fish (*Auchenaspis Salteri*) are so abundant in these beds, that I have seen in Henry Brooks's possession as many as four heads upon a slab a foot and a-half in diameter. The tail and body of this little fish are unknown, probably because, like the little Pimelodi of the Ganges, the Auchenaspis was a ganoid fish that had the fore part of the body covered with strong plates, with the tail naked and unprotected. The Pimelodus is a strongly helmed fish, *with a naked body*, which burrows in the holes of muddy banks, from which it shoots its armed head, and arrests, as they pass, the minute animals on which it preys.*

These beds have also furnished portions of the fossil fishes Plectrodus, Cephalaspis (probably a new species), Pteraspis, and Onchus. Pterygotus occurs here, with its fossil spawn, *Parka decipiens*, also a good-sized Lingula, with vegetable, probably fucoid, remains. These beds are without doubt the equivalents of the rocks which have furnished so many fossil treasures to Mr Lightbody of Ludlow, the President of the Woodhope Field Club The Auchenaspis beds at Ledbury pass upwards conformably into a series of red marls, with pink, yellowish, and gray sandstones, containing plates of Cephalaspis and Pteraspis, which are undoubtedly the base of the Cornstone series of the Old Red Sandstone. The fossil evidence furnished by the passage-rocks at Ludlow is important when compared with that of the Ledbury deposits many miles apart. The Lower Ludlow rock of the Ludlow district has furnished the oldest known relic of a vertebrate animal, the ganoid plate of a placoganoid fish, the *Pteraspis Ludensis*. The Upper Ludlow deposits of the Ludlow district have also furnished plates of Pteraspides below the well-known bone-bed. At Ledbury we have not discovered the bone-bed, but in the overlying strata we find remains of the same species of Pteraspis, Auchenaspis, and Cephalaspis, which characterise the equivalent deposits at Ludlow; and these are associated with the same species of Lingula and Pterygotus which abound in the Ludlow deposits. These facts, when associated with that thorough conformability of the Upper Silurians of Ledbury with the overlying Old Red Sandstone already mentioned, are facts that a geologist will do well to remember when studying the section at the Ledbury Station.

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* See Hugh Miller's "Rambles of a Geologist," p. 288.

The stranger should take a guide who knows the distant scenery, and ascending the hill of the Frith Wood near the town, should study with the Ordnance Map the geography of the country towards the west. I have said that the whole of the Old Red Sandstone once lay above the Silurian rocks of Ledbury and Malvern, and covered up Silurian deposits as the present surface deposits of our ocean beds cover up the masses that lie thousands of feet below them.

The beds of the Old Red at Ledbury are the lowest of a series of strata on the borders of Herefordshire, which have been carefully estimated at a thickness of from 8000 to 10,000 feet.

The rounded hill on the left hand and north-west of Ledbury about a mile, is called Wall Hills Camp, and consists of a series of marls, impure limestone (Cornstone), and thick red and gray sandstones, the uppermost of which are, I am convinced, at least two thousand feet above the uppermost Silurian rocks. This Old Red (Cornstone) hill of Wall Hills Camp is well worthy of study and observation on commencing the investigation of the geology of the Old Red of Herefordshire; for in a wood opposite Ledbury may be seen old quarries of impure Old Red limestone, which, intervening as layers between the marls and sandstone, has preserved many a wooded hill of Herefordshire from the denuding force of waves and currents, which for numberless ages swept far and wide over the district before us.

The Cornstone beds of the Old Red of Herefordshire commence low in the series as thin, nodular, impure, limestone bands, charged with the remains of fish, as seen in the parishes of Cradley, near Malvern, Trimpley, near Kidderminster, and Leyster Pole, Leominster. These beds of impure limestone appear in different horizons of the Lower Old Red, interstratified with marls and sandstones; the highest band being the large sub-crystalline masses of the Brown Clee Hills, Foxley, Orcop, the Graig, Eyas Harold, and Whitfield. The sub-crystalline Cornstone underlies, where not denuded, thick masses of red, gray, and brown sandstone rocks, which constitute the strata of the Scyrrid and Black Mountains. On Wall Hills we trace the remains of an ancient British camp; and the Cornstone quarry furnishes the remains of the characteristic fish *Cephalaspis Lyelli.* The hills of Canon Frome, on the right hand, or the east, in travelling towards Hereford, consist of the same strata as the Wall Hills, having resisted the denudation which has scooped out the intervening valleys down to the lower marls.

For the whole distance from Ledbury to Hereford the railway traverses strata appertaining to the beds of the Lower Old Red Sandstone, the zone of Cephalaspis and Pteraspis in Herefordshire, and of Cephalaspis in Scotland. For the further examination of the beds of the Cornstone series, the geologist must examine the hills around Hereford, and then proceed to the Scyrrid, Blorenge, and Sugar Loaf, near Abergavenny, and to the Daren and Pen-Cerrig-Calch, near Crickhowel. Here the brownstones, marls, and chocolate sandstones, that overlie the upper Cornstones, may be studied to advantage, as well as the uppermost beds of the Old Red; the conglomerate and yellow and gray sandstones which are seen pass upwards at the Daren and Pen-Cerrig-Calch into the Carboniferous shales, the base of the Carboniferous rocks.* The stranger must not suppose that the long range of hill he passes on the left (west), after leaving Wall Hills, belongs to the series of Old Red deposits. This range of hill, at the base of which is situated Stoke-Edith Park, the seat of Lady Emily Foley, is an upcast of Upper Silurian rocks, which westward, at Woodhope, forms a kind of dome, the top of which is denuded, the outside rim being presented towards the valley of the Hereford and Worcester Railway. The strata at Tarrington and Stoke-Edith are the equivalent rocks of the passage-beds of Ledbury. Shucknell Hill, on the east (right hand) of the valley of the Frome, is another Silurian upcast, and presents a good escarpment of Aymestry rock quarried for road stone.

At Bartestree, Lugwardine, a dyke of greenstone or diorite is seen to traverse and alter the Lower Old Red Sandstone, the Old Red beds being roasted into hornstone. A few fields farther north, near the house called Hagley Park, we may see a very good section, exhibiting the passage of the Upper Ludlow rock, with its bone bed, into the yellow Downton beds, succeeded by red marls precisely similar to those in the Ledbury Tunnel.

At Lugwardine, which the railway leaves a short distance to the west, there are extensive quarries of red sandstone of the Lower Old Red which afford fragments of the plates of Pteraspis, Cephalaspis, and a few triturated relics of plants, probably sea-weeds.

The city of Hereford stands principally on an alluvial gravel of the ancient Wye, from underneath which the Lower Old Red rises on every side. As in the vale of Severn, there is evidence that lakes have been silted up, and river currents have changed their courses, since the present configuration of the surrounding land. The Lugg Meadows, near Hereford, the Frome Meadows, and the great bend of the Wye between Rotherwas and the confluence of the Lugg, were formerly lakes now silted up by the ancient Wye. The Wye, however, is a much more rapid stream than the Severn, and destroys its banks far more rapidly, thus forming, destroying, and reforming river alluvium. I have yet to examine carefully the chronological history of the Wye, as compared with that of the Severn. A careful paper, by Mr Charles Richardson of Ross, appeared in the "Edinburgh New Philosophical Journal" some years ago (July 1857), which deserves the careful attention of geologists. I know something of the Wye and its alluvium, though not sufficient to enter into the minute details requisite for Mr Richardson's calculations. These calculations are carefully summed up in his "Chronological Remarks on the River Wye;" and the results arrived at are, that the Wye has flowed over the Old Red rocks of Herefordshire *for more than eleven millions of years.* We do not possess the same fossil evidence of the antiquity of the valleys of the Wye and Lugg as we do of the antiquity of the Severn and Avon vales. We are deficient in our relics of rhinoceros, hippopotamus, and the ancient ruminants; nevertheless the physical geologist doubts not that the valley of the Wye is at least as old as the period of the Severn lakes. We thank Mr Richardson, therefore, for giving us in full the items on which he has based his calculations, and we have reason to think that the rate of the wearing away of land would certainly not be found more rapid than he has estimated; unless we call in the aid of some catastrophic phenomena, of which there is not a tittle of evidence, if we have read aright the history of the ancient Wye.

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* See the author's papers "Edin. New Phil. Jour.," Oct. 1856, April 1858, and April 1859.

Observations on Temperature in connection with Vegetation, having special reference to the Frost of December 1860. By J. H. Balfour, A.M., M.D., F.R.S., Professor of Botany. *Including a Report on the Effects of the late Frost on the Plants in the Royal Botanic Garden of Edinburgh.* By Mr James M'Nab, Superintendent of the Garden.*

Meteorology has important bearings on various departments of natural science, and it has special relations to botany. Heat, light, moisture, and air are essential for the various phenomena of vegetable life, whether viewed in connection with the individual plant, or with the distribution of genera and species over the globe. It is of great importance, therefore, that accurate meteorological observations should be made in our botanical institutions. Our gardens ought to be supplied with good instruments, and our gardeners trained to accuracy in observing. The ordinary instruments required are of simple construction, and for their examination no great amount of skill or expenditure of time is demanded; and for working out the conclusions to be derived from them, none but the simplest rules of arithmetic are needed.

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* The substance of this paper was communicated to the Meteorological and Botanical Societies of Edinburgh.

Dr Hooker, in the "Gardener's Chronicle" for 1856, remarks —"Hitherto, the very facility of observation, the simplicity of the formulæ, and the cheapness of the instruments, have too often proved obstacles to the progress of meteorology, from having led to carelessness in the observations. The thermometers in common use are faulty to a proverb; and it is not too much to say, that in nine cases out of ten no one attaches any confidence to the temperatures recorded by gardeners, though there is no reason whatever why these should not be as accurate as a record of Greenwich Observatory. The thermometer itself is so cheap that its goodness is seldom inquired into; and it is, moreover, often imagined that the relation of the mercury to the scale is analogous to that of the magnetised end of the compass-needle to its card, and that as all magnetised compass-needles must always point to the magnetic north, so all columns of mercury must stand at the same height at the same time, if placed in reasonable proximity to one another. As little regard is paid to the reading off as to the selection of the instrument, and the fact that it often makes several degrees of difference in the result, whether the observer looks from above or from below to the point of the scale where the mercury stands, is very often disregarded. So, too, with regard to the position of the instrument—its exposure and height from the earth, provided it is out of the sunshine, few care where it is put. Paradoxical as it may seem, it is not the less true, that were the thermometer a costly instrument, and one troublesome to consult, the science of meteorology would now be far advanced beyond its present stage, and few would have bought any but a good instrument, or failed to place it in a good position, to observe it accurately, and to work out the results conscientiously. Nine-tenths of the observations would then be available for science. The correctness of these statements is seen in the records of unusual temperatures, like that which has occurred recently. We find places not a mile apart showing differences of temperature of 10° or more, at times when the temperature is steadiest."

It is to be hoped that the efforts of the Meteorological Society of Scotland will lead to a better state of things. Accurate instruments are now procurable at a moderate price (although not at the price usually paid for a gardener's thermometer). Simple rules for placing and observing the instrument have been drawn up, and there is every reason to believe that we have a staff of careful observers in various parts of Scotland.

Thermometers are usually placed at 4 feet above the level of the earth. This shows the temperature to which many bushes are exposed, but it does not give that which every herbaceous plant experiences, nor that to which the leaves, flowers, and fruits of trees are subjected. The temperature of the surface of the soil is occasionally very much lower than that above it, as shown by dew and hoar-frost. Hooker remarks, that "though the observations of Marcet of Geneva, and of Principal Forbes, have shown how the temperature varies at different elevations and under different circumstances, data are wanting from which any general laws may be deduced for calculating the rates of diminution at different localities, seasons, and hours, from the surface of the soil up to 300 feet above it; at about which elevation the diminution due to the action of the soil itself is no longer appreciable."

The difference of temperature at different levels is well shown by the observations of Quetelet at Brussels:—

Mean Temperature of Year.

At level of the soil, . .	46°·6	difference 3°.
At 10·8 feet above it, . .	49 ·6	

Mean Temperature at 9 A.M.

	Surface.	10·8 feet above.	Difference.
Winter,	34°·6	34°·4	−0°·2
Spring,	44 ·1	49 ·3	+5 ·2
Summer,	58 ·8	64 ·4	+5 ·6
Autumn,	48 ·6	50 ·5	+1 9

Maximum Temperature at 9 A.M.

	Surface.	10·8 feet above.	Difference.
Winter,	46°·9	52°·0	+5°·1
Spring,	57 ·9	67 ·6	+9 ·7
Summer,	70 ·0	79 ·5	+9 ·5
Autumn,	58 ·6	65 ·3	+6 ·7

Minimum Temperature at 9 A.M.

	Surface.	10·8 feet above.	Difference.
Winter,	20°·7	7°·3	−13°·4
Spring,	35 ·4	33 ·1	− 2 ·3
Summer,	52 ·0	51 ·3	− 0 ·7
Autumn,	37 ·0	34 ·5	− 2 ·5

Thus the height at which thermometers are placed has a very material effect on their indications.

In Brussels the thermometer at 10·8 feet elevation stands 5°·6 above one placed on the surface of the soil, and in winter a little below it; and at extreme temperatures, where the observations differ most and are most commented upon, the upper thermometer in summer and autumn stands as the mean 9°·7 above the lower, and in winter 13°·4 below it.

Decandolle says, that according to Marcet's observations at Geneva, the temperature ordinarily increases with the elevation above the ground, and that this difference is greater in summer than in winter, but that, nevertheless, in particular cases of extreme cold, the difference is enormous; as, for instance, on 20th January 1838, when an elevated thermometer stood 14° above another 50 feet below it. In continental countries, the differences are greater than in islands, because of the clearness of their skies, and their more variable temperatures. It is obvious, then, that a tree, herb, or shrub, growing side by side, are exposed to very different degrees of temperature. This no doubt invalidates the means of comparison of many facts which have been accumulated as to the effects of frost; "for," as Hooker again remarks, "putting aside the errors of instruments and observation, and the local disturbing causes that affect every place, it follows that no available information is given by such facts as that at Brighton a temperature of 18° killed such and such a plant, whereas in London a temperature of 10° did not affect it; unless the height of the individuals be given, their elevation above the main level of the soil if planted on mounds, and that of the thermometer also."*

Decandolle thinks that, as the height of the same species is much the same everywhere, whatever correction is to be applied to the observed temperature will probably not differ much in different countries. It is not, however correct, to state that the heights of the same species are usually the same. They differ often by several feet, and this may make a difference in the temperature.

My friend M. Charles Martins has recently made experiments in the Botanic Garden at Montpellier, on *the nocturnal temperatures.* We shall give some of the results. Agriculturists have long known that delicate plants, and even cereals, suffer much more from cold in valleys, or in depressions of the soil, than on eminences or on slightly elevated hillocks.† Six, Pictet, Marcet, Bravais and others, found experimentally, that during the night, and about an hour before sunrise and sunset, the temperature increases with the height in the lower atmospherical strata. They found that the increase was more marked the clearer and calmer the atmosphere.

M. Martins, in place of studying the nocturnal increase of temperature at a given moment, tried to ascertain what was the distribution of the greatest cold during the night—that is to say, the minimum. For a period of fifteen months he experimented during ninety-seven serene calm nights without rain. Six alcoholic thermometers, with uncovered bulbs, and radiating freely in all directions, were placed at heights varying from

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* The facts and observations above recorded are the substance of a paper by Dr Hooker in the "Gardener's Chronicle" for 1856.

† In regard to the plantations on the Nilgiri Hills, Dr Cleghorn states:—"The frost is detrimental to the young trees situated in the low lying swampy ground, but it seldom affects those planted on the slopes."

2 inches above the ground to about 160 feet above it. Four of the thermometers were fixed on a mast or flagstaff painted black, at heights from 2 inches to 19 feet 8 inches. A fifth was placed on a flat roof of a tower, at the height of about 83 feet; and the sixth was placed on the steeple of the Cathedral, at the height of about 160 feet above the first. During ninety-seven nights, M. Martins found only nine in which the temperatures decreased with the height. He considers, therefore, the rule to be, that during the night the temperature of the lower atmospheric strata *increases* with the height; and he looks upon the decrease of temperature as the exception.

For eighty-eight nights, during which he marked the increase, the mean thermal excess of the thermometer at 160 feet above that close to the ground was 4°·36. In the nights of decrease, the mean thermal decrease of the upper thermometer was only 0°·62. The mean increase between 2 inches from the soil and 6 feet 6 inches was 0°·39 for every 3 feet 3 inches; but between 19 feet 8 inches and 85 feet it was only 0°·07 for every 3 feet 3 inches; and between 85 and 160 feet it was 0°·02. The nature of the sky, whether clear or cloudy, and the nature of the air, whether calm or the reverse, have a powerful influence on the phenomenon. During serene nights, the mean increase has been noted at 5°·26 for 160 feet, or 0·11 for 3 feet 3 inches. In cloudy nights, the increase was only 1°·07 for the same difference of level. On 7th November 1859, which was very serene and calm, M. Martins noticed an increase of 8°·9. The increase has often been 7° or more, while the decrease was seldom more than 1°.

M. Martins gives the following illustration of the difference between thermometers at different heights: The Meteorological Observatory of the Faculty of Sciences at Montpellier is about 98½ feet above the point where observations are made in the Botanic Garden. The horizontal distance of the two stations, which are both on the north aspect of the hill on which Montpellier is built, does not exceed 1500 feet. The instruments are placed in similar conditions. Nevertheless, the minimum mean of 1859 was *lower* by 2°·91 in the Garden than at the Observatory. The maximum mean of the garden was actually higher by 0°·85. The mean temperature of the year for the two stations, deduced from the maxima, differ by 1°·03; which may be said to indicate, that 98½ feet in height in the south of France is equivalent to a difference in latitude of about 2°·40 in favour of the more elevated station.

How great errors, then, may be committed when we compare different stations, some situated in towns and others in the country; some exposed to one wind, some to a contrary one; some on the top of an elevated building, others on the causeway.

The hours of 7 A.M. in summer, and 8 A.M. in winter, adopted by the "Moniteur," seemed to Martins to be badly selected. At 8 A.M. in winter, for instance, the sun has risen for some time to the observer in mid-Europe, while it has not risen to the observer in the north. The former observes during the period at which the temperature decreases with the height, the latter while the nocturnal increase continues. There is nothing, therefore, comparable in the physical condition of the temperature of these hours at different degrees of latitude. He considers midday and midnight as the only times of the day which present everywhere some similarity in this respect.

These facts seem to have been verified by what has been noticed during the recent frost. In many elevated situations the recorded temperature has not fallen so much as in low situations. Mr Milne Home remarks:—"Low-lying parts of the country seem to have experienced the cold in considerably greater intensity than the more elevated districts. For example, at Braemar and at Castle Newe, in the upper portion of Aberdeenshire, the lowest temperature experienced was 8° and 11°, while, in the lowest stations, the average minimum was *minus* 6. In East Lothian, the two lowest stations of the Meteorological Society—Smeaton and East Linton—were also the lowest in respect to temperature; while at the most elevated—Yester and Thurston—the thermometer never fell below zero. The same difference was shown in an equally or even more remarkable way in Dumfriesshire. In the lowest parts of the country, the thermometers fell below zero; but at the Wanlochhead Station, which is 1333 feet above the level of the sea, the lowest marking was *plus* 6. The lowest temperature observed in any part of Britain was at Nottingham, which lies low, and is far from the sea. The thermometer there indicated –11° at four feet above the ground, and –13° upon the grass.

Decandolle thinks that, as regards botanical meteorology, thermometric measurements should be made by observations on the plants themselves, and not merely in the air. We are apt to look, he says, upon the presence of a certain vegetable form as being an accurate indication of climate, and each plant individually as a kind of thermometer. A plant, however, is a very complex machine, which carries on various processes under the influence of external agents, such as heat and light, and of an internal agent, life. The forces which set the plant-machine in motion are various, the details of its organisation are in part unknown, and its products, that is to say leaves, flowers, fruits, seeds, starch, sugar, are as numerous and varied. Hence there is great difficulty in drawing accurate conclusions. A certain minimum of heat is required for germination, another for some chemical modifications, a third for flowering. Then a certain sum of heat is required for each function, a certain intensity of light to give the green colour, and a certain quantity of water for most of the phenomena of plant life.

The plant, then, is not influenced by external agents in the same way as a thermometer is by temperature, or a hygrometer by moisture. Below a certain temperature a plant produces nothing, gives no signs of life, while the fluid of a thermometer continues to rise or fall. Every time the heat diminishes, the thermometer falls; while the plant, on the contrary, like a true machine, does not destroy what it has produced. It may be arrested in its growth, but the germ does not go back to the seed, nor the leaf to the bud, nor the flower to the stalk. We must therefore regard the phenomena of vegetation in a different light from mere physical indications.

Each species of plant can bear a definite range of temperature, and there is a certain mean which suits it best. Under favourable circumstances a plant will endure, without injury, a wide range of temperature above and below the mean which usually suits it best.

"In the Himalaya," Hooker states, that "on an average a large proportion of the species have a range of 4000 feet in elevation; this corresponds to a difference of mean temperature of 13°. Take, for example, a tree growing at from 9000 to 13,000 feet, as the Himalaya larch; it is exposed to mean annual temperatures of 44° at the lower, and 36° at the upper limits of the range; and as the mean temperature of the hottest month at 9000 feet is about 55°, and that of the winter at 13,000 feet is 18°, the species is exposed to a range of 37° between these two extremes. Farther, the difference between the mean maximum of 9000 feet and the mean minimum of 13,000 feet is 24°, to which difference of temperature two specimens of the same species may be exposed under ordinary circumstances on the same mountain. This is independent of the effects of exceptional temperatures, and of solar and terrestrial radiation, which would far more than double the amplitude. Yet the Himalayan larch has proved most difficult to cultivate in this country, and the scarlet rhododendron, which has a vertical range of nearly 8000 feet (or about double that of the larch), is still more difficult to cultivate in the open air in Britain."

Boussingault stated, that many plants, especially cultivated ones, required a certain heat for their perfect development; that between the time of the germination of the seed and the ripening of the fruit a certain amount of heat must be supplied. He measured this heat by taking the daily mean temperature, and multiplying it by the number of days. Where the heat was greater the plant was perfected in a smaller number of days, and where the heat was less a greater number of days was required; and he enunciated the statement, that if we multiply the number of days (the length of time the culture of a summer plant endures) by the mean temperature of this time, the product will be nearly the same in all countries and in all years. Thus, if a plant, he says, has taken twenty days to ripen its seeds from the period of flowering, and the mean temperature during these twenty days has been 10° Cent., it will be found that the heat received by the plant has been 200°. Again, the same degree of heat may be given in a smaller number of days by a greater temperature. Thus, if the mean temperature was 16° Cent., then the 200° of heat would be given in about 12½ days. Thus, he assumed, that each stage of vegetation, viz. from sprouting to flowering, and from flowering to fruiting and seed ripening, required a fixed amount of heat, and that, consequently, with a varying temperature, the time in which each was accomplished must likewise vary. He proposed to ascertain the sum of heat required for their different stages, and to draw conclusions as to the periods required for them in different countries, the mean temperature of which was known.

Thus he says, *Iberis amara* requires for flowering from 850° to 875° Centigrade; *i.e.*, if the mean temperature of all the days, from the sowing of the plant till its blossoming, be added together, we would make up that number of degrees of warmth. The number of days, according to the nature of the spring, will vary from 58 to 66. Again, from the flowering of the plant to the ripening of the fruit 1000° of heat are required, distributed over 52 to 62 or more days.

In Alsace, he found that barley would be perfected in 92 days, where there was an average temperature of 19° Cent., giving a total of 1748° of heat. He then obtained the data from Cumbal, situated under the equator, where the plant was grown between June and November instead of between May and August. The daily temperature was there (it being in the colder season) from 10° to 11° Cent., and 168 days were required for the perfecting of the plant, giving a total of 1797°—a number nearly corresponding with the other. In many cases the numbers came out very satisfactorily for his hypothesis. It was found, however, that there were many errors in the mode of computation. Boussingault did not pay attention to the omission of useless heat—that below the freezing of water at all events. As water is the medium through which nourishment is conveyed to plants, we must certainly not commence reckoning the degrees of heat with the days until the temperature is above that of freezing. Moreover, many plants do not begin to exercise their vital functions until the temperature is several degrees above that point. All plants do not vegetate at the same temperature. In some, the sap begins to ascend when the temperature is only a few degrees above 32° Fahr.; others require an increase of 20° or 30°; and plants of hot climates require 60° or 70° Fahr. Every plant is its own thermometer, whose zero corresponds to that temperature below which its vital functions are suspended. In ascertaining the sum of temperatures which determines flowering and fruiting, we should only take those which are above the zero of the species, for this alone stimulates the plant to action.

M. Fintelmann says—"Putting aside the facts, that in the same day it may be warm at certain hours and freeze at others, that plants will grow in the sunshine at the same time that others are checked in the shade, and that errors have arisen from overlooking these facts; yet each plant requires, in addition, an entirely different amount of heat to bring it to perfection. For example, our willows and poplars will sprout at a low temperature, while plants from foreign countries, as the tulip-tree, need many degrees of uninterrupted heat before the buds begin to unfold. The seed of the *Capsella Bursa-pastoris*, common shepherd's purse, sprouts under the snow as soon as the temperature rises above the freezing point; while wheat requires about 6°, and flax about 8° of heat before their seeds begin to swell. Were the assumption correct, that all plants sprout as soon as water begins to liquefy, then the result would be that all tropical plants would stand the open air with us during those months in which it does not freeze. That is known not to be the case; each plant needs for the preliminary processes within its cells—in other words, for its nourishment—an amount of warmth peculiar to itself."

While *Iberis amara* requires few degrees of heat at first, flax will not sprout until the temperature is considerably higher. When sown at the end of April, flax required for its entire development 101 days, with 1605° of warmth; when sown at the end of May and June, its existence was concluded in 75 or 71 days, with 1272° and 1757° of heat. If *Iberis amara* is sown at the end of April, it needs for its entire course of existence 110 days, with 1754° of warmth; if sown at the end of May, it requires 109 days and 1821°; should the sowing be postponed till 24th June, it needs 124 days and 1858°.

Again, the direct light of the sun exercises an essential influence on vegetation. If grown in sunshine, *Iberis amara*, before flowering, requires 58 days, with 827° of heat; and until the ripening of the seed 52 days, with 927°—thus making a total of 110 days and 1754°; while, if grown in the shade, it requires 66 days, with 950° of heat before blossoming; and 73 days, with 1265° of heat before ripening its seed; altogether, therefore, 139 days and 2215° of heat.

It must also be borne in mind, that too high degrees of heat act injuriously, and cause disease. In tropical countries apple and pear trees seldom reach maturity, and it is not uncommon to find plants grown in houses becoming altered, so as not to produce fruit, even although they flower freely. Palms, acacias, and other plants, which have been subjected to high temperatures for years in a hothouse, often have barren flowers. There is thus a limit to the supply of heat as regards the functions of plants. This requires to be kept in view in our attempts to carry out Boussingault's formula.

Another important point omitted by Boussingault is the temperature of the soil. This operates powerfully on plants. With a high temperature of the soil, plants may be seen flowering and bearing seed in this country which rarely fruit in ordinary circumstances. What is termed Geothermal culture has of late years attracted much attention, and every one is now satisfied of its importance. Bottom heat is essential for the successful cultivation of plants in our hothouses. If palms are planted out in our houses, instead of being placed in tubs, care must be taken that their roots are supplied with heat. If these extend into the ordinary cold soil, the life of the plants will often be destroyed.

The presence of manures and other matters in the soil is in some cases beneficial, by giving rise to the production of heat. It is by no means uncommon to see a difference in the earliness of flowering produced by a difference in the warmth of the soil. The winter temperature of the soil, when covered with snow, is in many cases above the freezing point when melting commences, and is considerably augmented before the dormant vitality of plants is aroused and germination of their seeds begins.

Hooker says that at Dorjiling in the Sikkim Himalaya, lat. 27 N. and alt. 7000 feet, the temperature of the earth at 2 feet 7 inches beneath the surface was in December and January 5° above the monthly mean; in November and February, 4° to 5°; in March a few degrees below the mean temperature of the month; in October above it; April and May being sunny, it stands above the mean; June to September a little below the mean temperature of each respectively.

The mean temperature of the soil at 2 to 3 feet is almost throughout the year in India above that of the surrounding atmosphere. This is also shown to be the case in England; and Mr Thompson found at the Horticultural Garden, Chiswick, that the temperature of the soil was, on a mean of six years, at a depth of 1 foot, 1° above that of the air; at 2 feet, 1½°. During the winter months the soil was from 1° to 3° warmer than the air, and during summer it was a fraction of a degree cooler than the air.

In India, the mean temperature of the soil in winter may be often 9° to 10° above the air. The accumulated heat in the upper strata of the soil prevents delicate rootlets from becoming frozen, and preserves vitality in some fleshy roots, such as rhubarb and orchids. The temperature is also useful to burrowing Rodentia, as hares, marmots, and rats, which abound at 15,000 to 17,000 feet in Tibet. The rapid development of vegetation after the disappearance of snow is no doubt also proximately due to the heat of the soil, quite as much as to the increased strength of the sun's direct rays in lofty regions.

The temperature of the soil in alpine regions accounts for the growth of plants under snow. M. Martins states that the results indicated by a thermometer inserted in the soil on the summit of Faulhorn, about 8790 feet above the level of the sea, and noted for several weeks by Bravais, Peltier, and himself, have enabled them to promulgate the law, that in the plain the temperature of the surface of the soil is in general below that of the air, while on high mountains the soil is markedly warmer than the air. This relative warming, so observable on the surface of the soil, exercises a powerful influence on the physical geography of the High Alps. It raises the snow limit by causing the melting of the snows.

Travellers in these lofty regions find that the melting proceeds chiefly on the lower surface, and is due mainly to the heat of the ground. When the foot is placed on the edge of a snow-field, the weight of the body often breaks the superficial crust, which is found not to be in contact with the soil below, and there are often displayed under an icy vault specimens of *Soldanella alpina* and *S. Clusii* in flower, or the rosettes of the leaves of the common dandelion. This is not the case in Spitzbergen, where the edge of the snow-field lies always closely on the soil. The slipping of fields of snow, and the occurrence of spring avalanches, depend upon the melting of the lower surface of the snow from the heat of the earth communicated to it. Herbaceous plants, in these alpine districts, send their roots into the warm superficial layers of the soil, and thus are enabled to thrive. The dark colour of the vegetable soil favours the absorption of heat. On the terminal cone of the Faulhorn, the height of which is about 262 feet, and the superficial extent about 11 acres, Martins gathered 131 phanerogamous species; while the entire island of Spitzbergen, which has a far larger superficies, contains only 82. At the Grands Mulets, schistose protogenic rocks which rise in the midst of the glaciers of Mont Blanc, at a height of about 10,000 feet above the level of the sea, and consequently about 1100 feet above the limits of perpetual snow, Martins collected the following plants, *Draba fladnicensis*, Wulf.; *Cardamine bellidifolia*, L.; *Silene acaulis*, L.; *Potentilla frigida*, Vill.; *Phyteuma hemisphæricum*, L.; *Erigeron uniflorus*, L.; *Pyrethrum alpinum*, Willd.; *Saxifraga bryoides*, L.; *S. grœnlandica*, L.; *S. muscoides*, Auct.; *Androsace helvetica*, Gaud.; *A. pubescens*, DC.; *Gentiana verna*, L.; *Luzula spicata*, DC.; *Festuca Halleri*, Vill.; *Poa laxa*, Hæncke; *P. cæsia*, Sm.; *Agrostis rupestris*, All.; *Carex nigra*, All. He also found on 28th July 1846, that when the temperature of the air in the shade was 50° Fahr., and in the sun, 58·5° Fahr., that of the soil in which the plants were growing was 84°·2 Fahr. At great alpine elevations, as on the Faulhorn, Rothhorn, in the valley of Urseren, and on the Grands Mulets, a kind of mouse, called *Arvicola nivalis*, was found by Martins to live by burrowing in the soil.

The growth of plants under snow was observed recently *in* the Edinburgh Botanic Garden. Two Umbelliferous plants, *Ferula campestris* and *F. tingitana*, during the four weeks that the snow was on the ground grew seven inches.

The nature of the strata or layers of soil has much effect on vegetation. When the substrata are formed of chalk and marl, the surface soil will imbibe, according to Fintelmann, far less of the central heat than if volcanic or plutonic rocks—as basalt, trachyte, porphyry, and the like—lie below it, since the conductibility of the latter is much greater than that of the former. Hence mountains composed of porphyry and the like are warmer, on an average, than those composed of chalk and marl,. and even of clay-slate; and possess also a more luxuriant vegetation. The chasms and rents in volcanic countries also assist the transmission of subterranean heat. This reason seems in part to explain the comparatively luxuriant vegetation of the valleys and meadows of Switzerland and the Tyrol.

The oblique manner in which the rays of the sun reach the earth in northern climates prevents the full effect of heat on the soil. Fintelmann says, upright windows in conservatories transmit the greatest amount of heat in winter, but obstruct the passage in summer; those, on the contrary, which slope at an angle of from 40 to 45 receive nearly the whole force of the sun's rays in summer, and thus transmit a greater amount of heat to the interior of the building, while in winter it is precisely the reverse—the sun's rays enter obliquely, and the heat is less.

The operation of Draining is of great importance in adding to the heat of the soil. Much injury is inflicted on the soil by stagnant water. The land is rendered cold, inasmuch as the sun's rays, in place of being expended in heating the soil, are absorbed by the water, the temperature of which is not raised so rapidly as that of the earth or of the air. There is thus a great loss of heat. Drained land in summer is from 10° to 20° warmer than water-logged land. Professor Schubler states, that the loss of heat caused by evaporation in undrained lands amounts to 11½° to 13½° Fahr.

Parkes says, that in draining the red moss, near Bolton-le-

Moor, the thermometer in the drained land rose in the summer of 1837 to 66° at 7 inches below the surface, while in the neighbouring water-logged land it never rose to above 47°. In the London Horticultural Society's garden, the mean temperature of thoroughly-drained soil at one foot below the surface in the month of July is 63°·49; and if we take water-logged land to be the same as spring water (47°), then there is a gain of 16½°. Experiments have been made which show, that in peaty soils, saturated with water, the addition of boiling water to the surface will not raise the temperature to any depth in the soil, so that wet land will thus remain cold, in spite of all the warm rain that falls. This is not the case with drained soil, where there is a constant percolation.

In rainy and cold seasons there is great necessity for thorough drainage, both surface and underground. The means of surface drainage should be at command when required; but by deepening the soil when it is too thin, and rendering it permeable to water, with deep under-drainage to prevent stagnation, most of the summer rain that falls can find its way through the soil, rendering it comparatively warm. But where under drainage is neglected, the accumulated moisture stagnates on the already saturated soil, which is cold, in consequence of the winter rains or melted snow having never been drawn off. That moisture, from its coldness, is heavier than the warmer summer rains. The latter may run off by the surface, and thus their warming effect on the soil is lost, but they cannot displace the colder and heavier water, which consequently retains possession of the soil for the greater part of the growing season. In that case the crops cannot thrive as they otherwise would, and they are, besides, rendered late, so that in bad seasons they are in danger of being lost. (*Gard. Chron.* 1860.)

The leafing (foliation), flowering, fruiting, and defoliation of plants, are intimately connected with climate and seasons, and they supply important data as regards temperature and other meteorological influences. Berghaus, Quetelet, and Hartmann, have given tables of foliation and defoliation of the same trees in different parts of Europe, and have deduced from these phenomena important meteorological conclusions.

It is of course necessary to compare the same species in different countries, if we wish to draw correct inferences. Elevation, exposure, and soil, must also be taken into account. The epochs of plant-flowering engaged the attention of Linnæus, and a floral calendar was constructed by him for Upsal. Similar records have been kept in different places, and the results have been recorded by Göppert, Schubler, and others. In the Edinburgh Botanic Garden, Mr M'Nab for many years kept a register of the flowering of spring plants, selecting year after year the same specimens, in the same localities, and with the same exposure.

The Table on the opposite page (249) contains a selection of the plants whose flowering has been thus recorded.

Such observations should be accompanied with thermometric measurements, both in the air and in the soil, in fixed situations. The subject of the registration of periodical phenomena, both in the case of plants and of animals, has been taken up by the British Association; and the committee appointed by that body have given lists of plants which are to be observed for the periods of leafing, flowering, ripening of the fruit, and fall of the leaf, as well as for the opening and closing of the flowers. The Vienna naturalists have also propounded a similar list of plants for continental countries. The following is their list:—

Acer platanoides, *L.*
Æsculus Hippocastanum, *L.*
Berberis vulgaris, *L.*
Catalpa syringifolia, *Sims.*
Colchicum autumnale, *L.*
Convallaria majalis, *L.*
Cornus mascula, *L.*
Corylus Avellana, *L.*
Crocus vernus, *L.*
Cytisus Laburnum, *L.*
Daphne Mezereum, *L.*
Fagus sylvatica, *L.*
Fraxinus excelsior, *L.*
Fritillaria imperialis, *L.*
Hepatica triloba, *L.* (cærulea).
Hordeum vulgare, *L.* (hybernum and æstivum).
Leucojum vernum, *L.*
Lilium candidum, *L.*
Prunus Avium, *L.*
Prunus Padus, *L.*
Pyrus Malus, *L.*
Ribes Grossularia, *L.*
Ribes rubrum, *L.*
Robinia Pseudo-Acacia, *L.*
Sambucus nigra, *L.*
Secale cereale, *L.* (hybernum and æstivum).
Pyrus Aucuparia, *L.*
Syringa vulgaris, *L.*
Tilia parvifolia, *Ehrh.*
Triticum vulgare, *L*, (hybernum).
Vitis vinifera, *L.*

They recommend that observations should be made on the

Register of the Flowering of Spring Plants in the Edinburgh Botanic Garden.

	1861.	1858.	1857.	1856.	1855.	1854.	1853.	1852.	1851.	1850.
Eranthis hyemalis, ...	Jan. 26.	Jan. 16.	Feb. 9.	Feb 14.	Mar. 2.	Jan. 26.	Feb. 1.	Jan. 31.	Jan. 15.	Feb. 14.
Crocus susianus, ...	... 28.	... 15.	... 15.	... 18.	... 5.	Feb. 14.	Mar. 8.	Feb. 3.	... 26.	Jan. 14.
Rhododendron atrovirens, . .	... 28.	... 5.	... 6.	... 16.	April 6.	... 18.	Feb. 1.	Jan. 14.	... 2.	... 14.
Galanthus nivalis, ...	... 31.	... 4.	... 8.	... 14.	Mar. 2.	Jan. 24.	Jan. 24.	... 28.	... 17.	Feb. 11.
Anemone hepatica, ...	... 31.	... 14.	... 13.	... 16.	... 7.	... 20.	Feb. 2.	...	...	...
Leucojum vernum, ...	Feb. 1.	Feb. 10.	... 24.	Mar. 1.	... 3.	Feb. 15.	Mar. 21.	Feb. 21.	... 20.	... 18
Dondia Epipactis, ...	... 1.	... 18.	Mar. 14.	... 10	April 9.	Mar. 11.	... 25.	Mar. 8.	... 4.	Mar. 2.
Sisyrinchium grandiflorum, .	... 5.	Jan. 14.	Feb. 14.	Feb. 26.	Mar. 5.	Feb. 14.	... 3.	Feb. 3.	... 27.	... 12.
Omphalodes verna, ...	... 6.	...	...	...	...	...	...	April 2.	Mar. 10.	... 22.
Pothos fœtida, ...	... 9.	Feb. 9.	... 28.	... 26.	... 20.	Mar. 3.	... 16.	Feb. 20.	Feb. 4.	Feb. 18.
Tussilago alba, ...	... 10.	Jan. 14.	... 20.	... 24.	... 15.	Feb. 14.	... 1.	... 27.	Jan. 26.	Mar. 12.
Corylus Avellana, . . .	... 10.	... 14.	... 16.	... 15.	... 21.	Mar. 10.	... 9.	Jan. 25.	... 16.	Feb. 16.
Crocus vernus and varieties, .	... 12.	... 18.	... 19.	... 24.	... 6	Feb. 4.	... 15.	Feb. 18.	Feb. 3.	... 26.
Erica herbacea, ...	... 15.	... 14.	... 6.	... 15.	... 5.	... 20.	Jan. 28.	Jan. 24.	Jan. 16.	...
Aubretia grandiflora, . .	... 18.	Mar. 6.	... 26.	Mar. 1.	April 8.	... 17.	Feb. 1.	Mar. 18.	Mar. 1.	Mar. 24.
Nordmannia cordifolia, . .	... 19.	...	... 27.	... 8.	... 9.	Mar. 1.	Mar. 24.	... 10.	Feb. 20.	Feb. 28.
Symphytum caucasicum, . .	... 24.	Jan. 18.	Mar. 3.	Mar. 12.	... 10.	... 11.	... 26.	Feb. 2.	Jan! 23.	Mar. 14.
Muscari botryoides, ...	... 26.	Feb. 17.	April 7.	... 30.	... 14.	... 24.	... 22.	Mar. 20.	Mar. 11.	... 18.
Anchusa sempervirens, . .	... 28.	Jan. 16.	... 4.	April 1.	... 13.	... 25.	Apr. 12.	Feb. 21.	Feb. 14.	Feb. 26.
Orobus vernus, ...	Mar. 4.	Mar. 11.	... 20.	... 4.	... 16.	... 16.	... 8.	Mar. 31.	... 17.	... 23.
Narcissus pumilus, ...	... 6.	... 12.	Mar. 15.	Mar. 16.	... 2.	... 10.	Mar. 21.	... 11.	Mar. 5.	Mar. 4.
Arabis albida, ...	... 6.	Feb. 16.	Feb. 13.	Feb. 24.	... 8.	Feb. 15.	... 15.	Feb. 15.	Feb. 7.	Feb. 21.
Primula denticulata, . . .	... 7.	... 25.	...	...	... 7.	... 27.	... 25.	... 19.	... 15.	... 23.
Scilla bifolia, ...	... 7.	Mar. 16.	Mar. 24.	Mar. 20.	... 10.	Mar. 15.	... 27.	Mar. 20.	Mar. 6.	Mar. 13.
„ sibirica, ...	... 8.	... 20.	Apr. 14.	April 7.	... 21.	... 30.	Apr. 10.	April 1.	... 26.	April 6.
Hyoscyamus Scopolia, . .	... 9.	... 19.	April 1.	Mar. 26.	... 15.	... 14.	... 4.	Mar. 25.	Feb. 24.	Mar. 1.

first appearance of the surface of the leaf; the first fully expanded blossom, the pollen appearing prominent; the first ripe normal fruit, without worm-holes, at the beginning of the harvest of each kind of grain; the general decoloration of the leaves; the duration of vegetation with reference to the weather,—viz., the first and last frost; and the duration of snow.

Certain peculiar phenomena in reference to the opening and closing of flowers should also be noticed. On this subject, interesting observations have been made by Fritzsch, as recorded in the eighth volume of the Transactions of the Horticultural Society of London. The following table shows that the temperature at which flowers begin to expand varies, and the time of the year has therefore a marked influence:—

Temp. Fahr. at which Species begin to expand.		Mean Time of Observation.
38°·75	Two species	April 3.
43 ·25	Five ,, ...	April 18.
47 ·75	Nine ,, ...	May 18.
52 ·25	Sixteen ,, ...	June 16.
56 ·75	Fourteen	July 5.
61 ·22	Six ,, ...	July 12.
66 ·75	Malva Alcea	July 7.

The following table indicates the temperature at which the greatest degree of expansion of the flowers take place:—

Temp. Fahr.		Mean Time of Observation.
47°·55	Mirabilis Jalapa (night-bloomer)	
56 ·75	Erodium cicutarium . . Lychnis vespertina (night-bloomer)	May 6.
61 ·25	Four species, ... Funkia japonica (night-bloomer) Œnothera biennis (night-bloomer)	Aug. 10.
65 ·75	Three species	July 2.
70 ·25	Eleven ,, ...	May 27.
74 ·75	Thirteen ,, ...	May 30.
79 ·25	Fifteen ,, ...	June 26.
83 ·75	Twenty ,, ...	June 29.
88 ·25	Thirteen ,, ...	July 13.
97 ·25	Echinocactus Ottonis . .	July 15.
106 ·25	Carlina acaulis	Sept. 1.

The night-bloomers reach their maximum of expansion at from 47°·75 to 61·25; the day-bloomers from 79°·25 to 88°.25.

The temperature at which all motion in the corolla ceases varies, as seen by the following list:—

In three species, motion ceased at			74°·75 Fahr.
In three	,,	,,	83 ·75
In five	,,	,,	88 ·25
In four	,,	,,	92 ·75
In seven	,,	,,	97 ·25
In Commelina cœlestis		,,	101 ·75

Thus some flowers cannot bear a temperature of 74°·75, while others can endure a temperature of 100° and more.

Plants, in ordinary circumstances, require the light of the sun in order to awake from sleep, either directly by insolation, or indirectly by diffusion in the atmosphere. Some are so sensitive that they begin to expand the moment the sun's rays illuminate the higher regions of the atmosphere. As the intensity of light increases, the number of species which expand their blossoms increases. The number of species increases as the hour approaches at which insolation (exposure to the direct rays of the sun) ceases. No blossom closes before the insolation begins, for even those flowers which usually close before mid-day remain for some hours open when exposed to the direct rays of the sun.

The subject of *Acclimatising*, as it is called, is brought under our notice, especially in a season like the present, when we have experienced a temperature lower than usually occurs in this country. Such seasons occurring occasionally test the hardiness of introduced plants; and we shall find that many trees and shrubs are injured or destroyed which have stood our climate, it may be, for forty or fifty years. It is clear that it is not safe to introduce into plantations trees which cannot stand the lowest amount of cold which may occur in the country.

It has often been stated that tender exotics may, by long cultivation, be made to bear the climate of Britain. It has been thought that they may become accustomed to it by degrees, and thus be acclimatised. Some have even hinted that by sowing the seeds of such plants, in the first instance, in a warm temperate region, then collecting the seeds produced by the plants, and sowing them in colder districts, the species may be rendered hardy. We are constantly told that plants which, when first introduced into Britain, were put into stoves and greenhouses, are now growing in the open border freely, and that they can stand a climate which they could not do at first. Such statements do not rest on a sound basis; each species of plant bears a certain range of temperature, and we cannot extend its natural limits. The plants said to be acclimatised were not tried in the open air when first introduced, otherwise they would have been found to be hardy, without any previous process of cultivation in greenhouses. The well-known shrub *Aucuba japonica* was treated in a stove when first introduced, and was afterwards planted out, and found to stand our ordinary climate. This was not an instance of acclimatising, but indicated an error regarding the constitution of the plant, which was brought from the colder parts of Japan, and was capable of enduring the ordinary cold of this climate naturally. Man did nothing in the way of changing its constitution and powers of endurance. This plant has suffered severely this season. There is no evidence that plants long cultivated in this country are able to withstand our winter cold better than formerly. The Dahlia, the Heliotrope, and the Potato, are affected in the same way by frost as when they were first introduced. Long cultivation has done nothing to increase their hardiness. When we consider that the great risk in this country is the accession of frost after a few genial days or weeks of spring, we may do something for the preservation of half-hardy plants, by planting them in such localities and soils as to prevent them from being stimulated too early, and from being attacked by frost when full of sap. We can put tender plants on elevated situations not exposed to the direct rays of the sun, and on well-drained soils, and thus prevent much injury. We may also, by means of geothermal culture and covering plants during winter, enable those in the open ground to live during our winters. Hardiness may also be imparted by hybridising and grafting. Tender trees or shrubs grown in warm and moist valleys, exposed to the sun's rays, are often injured by spring frosts. It is of great importance to define accurately when a plant may be said to suit a particular climate. It is not enough that it lives and sends out leaves, it must also be able to produce flowers and seeds, and to elaborate the peculiar secretions and products on which its qualities depend.

The injury caused by low temperature depends greatly on the suddenness of its accession, and upon the epoch of the plant's life, and the time of the year at which it supervenes. A sudden frost occurring in spring, after the sap has begun to flow, causes great injury. This occurrence is common in our climate, and is the cause why many half-hardy exotics require to be covered, or kept in a dormant state, until the season has advanced beyond the risk of frost. Many such plants grown, on a wall with a southern exposure, are stimulated by the heat of a fine day in early spring, and are thus unable to resist later frosts. Warm sun during the day, and frost at night, have proved fatal to many exotics introduced into Britain. An Asafœtida plant began to flower in the Botanic Garden in February 1858, and continued vigorous during March; but on the 13th April the thermometer suddenly fell during the night to 22° Fahr., and killed the plant, then full of sap.

The injury done to trees and shrubs in the Edinburgh Botanic Garden during the frost of December 1860 has been recorded by Mr M'Nab, and the following detailed account has been drawn up by him:—

The storm commenced late on the evening of the 18th December, with a very heavy shower of hail, lasting for some time, and succeeded by a quiet fall of snow, which continued throughout the night. At daylight on the morning of the 19th December the snow was 12 inches in depth all over the garden. On the morning of the 20th of December, about 5 inches of extra snow had fallen. For ten days the snow lay more or less heavy on all the shrubs, many being weighed down and broken by it. In some instances we may account for the damage done by the action of the frost on the overbent branches, particularly where a rupture was caused by the weight of snow. Irish yews 10 and 12 feet high had their various tops touching the ground. When freed of their load they soon assumed their original upright form, which would not have been the case had they been long held in a bent position. On the morning of the 23d December, the thermometer indicated 25° Fahr. The day subsequently was clear and cold, and there was every appearance of a sharp frost. The following tables show the minimum temperature indicated every two hours by Adie's and Bryson's thermometers during the three nights referred to. The thermometers were each placed about four feet from the ground, and were freely exposed to the atmosphere:—

Mr Adie's Thermometer.

	Dec. 23–24. Degs.	Dec. 24–25. Degs.	Dec. 25–26. Degs.
4 P.M.	13	9	7
6 ,,	10	4	3
8 ,,	8	3	0
10 ,,	6	3	−2
12 Midnight	3	3	3
2 A M	3	3	9
4 ,,	3	3	12
6 ,,	3	3	3
8 ,,	−2	3·5	−2
10 ,,	−5	2·5	−2

Mr Bryson's Thermometer.

	Dec. 23–24. Degs.	Dec. 24–25. Degs.	Dec. 25–26. Degs.
4 P.M.	10	6	4
6 ,,	7	4	0
8 ,,	5	0	−3
10 ,,	3	0	−5
12 Midnight	0	0	0
2 A M.	0	0	6
4 ,,	0	0	9
6 ,,	0	0	0
8 ,,	−5	0·5	−5
10 ,,	−8	−5	−5

It has been found necessary to apply a correction to the indications of very low temperatures on both thermometers, in order to bring them to the Kew standard. It was ascertained that near zero Adie's thermometer indicated 1° too high, while Bryson's indicated 2° too low. After rectification, the lowest temperature indicated in the Botanic Garden—namely, that which occurred at 10 A.M. on Monday the 24th December—was, according to the Kew standard, 6° below zero.

The following table shows the minimum temperature indicated by Bryson's thermometer (rectified as explained), during the nights from the 14th December 1860 to the 14th January 1861, both inclusive:—

	Degs.		Degs.
December 14,	29	December 30,	25
„ 15,	30	„ 31,	32
„ 16,	33	January 1,	32
„ 17,	31	„ 2,	20
„ 18,	30	„ 3,	15
„ 19,	27	„ 4,	8
„ 20,	28	„ 5,	8
„ 21,	24	„ 6,	16
„ 22,	18	„ 7,	19½
„ 23,	25	„ 8,	5
„ 24,	−6	„ 9,	18
„ 25,	1½	„ 10,	32
„ 26,	−3	„ 11,	30
„ 27,	5	„ 12,	36
„ 28,	4	„ 13,	28
„ 29,	14	„ 14,	25

After the intense frost gave way, the weather was, fortunately for vegetation, rather dull, with comparatively few glimpses of sunshine. The damage done to vegetation, however, is very great, and is daily becoming more apparent; but months must elapse before we shall be able to know the entire loss. The first plants on which the effects of the frost became visible were *Aucuba japonica* and *Viburnum Tinus* the common laurustinus. The leaves of the former became totally black, with the exception of those which were covered with snow round the bottom of the plants, while the leaves of the laurustinus became brown. During the thaw the latter emitted a very strong and disagreeable cat-like odour—the smell not being perceptible so long as the plant remained in the frozen condition. The next shrubs which showed the effects of the frost were the bay or cherry laurel (*Prunus Laurocerasus*), the Portugal laurel (*Prunus lusitanica*), and the sweet bay (*Laurus nobilis*). Among the other plants which suffered early were species of *Rhamnus, Phillyrea, Mahonia, Azara, Cydonia*, and certain varieties of hollies, particularly the golden and the leather-leaved. *Rhododendron ponticum* and hybrids from *R. campanulatum*, *Quercus Ilex* (evergreen oak), *Q. Suber* (cork oak), all suffered more or less severely.

Among the Coniferæ, *Araucaria imbricata* showed symptoms of injury, by the bending of the ends of the branches and a change in the colour of the leaves. *Libocedrus chilensis*, *Fitzroya patagonica*, *Cupressus torulosa*, *Pinus insignis*, *Abies Morinda*, and *Cedrus Deodara*, exhibited marked indications of injury, more especially such of them as were planted in the moister part of the garden.

During the last three or four weeks (February) the effects of the late frost are being seen on the evergreen trees and shrubs Many species at first apparently untouched are now found to be much injured, if not destroyed. The evergreen oaks (*Quercus Ilex*), especially in damp situations, will probably require to be cut down. One large specimen, 35 feet high, having a stem 6 feet in circumference, with a hemispherical top 135 feet round, is very much injured. The injury sustained by these oaks was first seen in their stems, and in the points of their shoots, and subsequently the intermediate portions became brown like the rest of the tree. The Lucombe oak (*Quercus Cerris*, var.) is injured, and shows the effects in the same way as the evergreen oak. In the case of the cork tree (*Quercus Suber*), although all the young shoots are gone, the branches and stems seem as yet untouched, evidently protected by the thick corky bark. The leaves of all the large plants of *Arbutus Unedo* have become black, and the bark on the lower parts of the stem has burst, and is coming off in flakes. The wood has also become dry and discoloured. It is now evident that all the specimens of Arbutus in the garden will require to be removed. Many of them are 15 feet in height, with fine tops 36 feet in circumference. All the plants of the scarlet *Arbutus Unedo* are likewise destroyed. This variety was considered hardier than the common species, a larger proportion of the plants having withstood the severe winters of 1837–38 and 1844–45. The *Arbutus Andrachne*, although less affected than the *A. Unedo*, is also much injured. One fine plant, 10 feet high, having a stem 2 feet 7 inches in circumference, with a globular head 45 feet round, seems in a precarious state; the leaves on the lower six feet are becoming discoloured. All the specimens of *Rhamnus Alaternus* in the garden, many of them 10 feet high and 30 feet in circumference of branches, have been killed. The *Laurus nobilis* (Sweet Bay) is also killed. The largest specimen was 6 feet high, and 18 feet in circumference of branches. The *Aucuba japonica* is killed to within 18 inches of the ground, the largest plants being 7 feet high, and 20 feet round. All the varieties of Laurustinus are killed to the surface of the earth, the largest plants being 8 feet high, and 18 feet round. The Portugal laurels (*Prunus lusitanica*), particularly those plants which had been freely pruned during previous years, are now parting with their dry leaves; while the free-growing unpruned specimens are becoming soft and black in their foliage, and, where growing in damp situations, the stems are very much injured. In the case of the bay or cherry laurel (*Prunus Laurocerasus*) many specimens have their stems totally destroyed, and, where not killed, the leaves are of a dull brown colour. The points of many of the deciduous trees have suffered much, such as the walnut (*Juglans regia*), sumach (*Rhus elegans*), also the oriental plane (*Platanus orientalis*), *Catalpa syringifolia*, *Pawlownia imperialis*, &c.; but the full extent will not be known till spring advances. The deciduous shrubs, particularly the early flowering sorts, have had their flower-buds much destroyed, such as *Azalea pontica*, *Ribes sanguineum* (in some situations), *Cydonia japonica*, *Deutzia gracilis*, &c. Roses have also suffered very much, all the climbing or wall-roses being destroyed. Amongst them may be noticed a fine specimen of *Rosa Burnoniana*, which was planted about 1826, and which has now a stem 9 inches in circumference. The tall standard-budded sorts are also killed. The mistleto (*Viscum album*), when growing on apple-stocks, and also on the branches of *Mespilus canadensis*, has been very much injured; but it is quite green and healthy when growing on oak and thorn.

The late frost, in situations where the thermometer fell below zero, has proved very injurious to the buds and young shoots of many fruit-trees. The injuries in many cases will not be sufficiently known till the shoots containing the blossom-buds expand. The buds are now (February) swelling fast, and, if cut longitudinally, the young flowers will be found quite brown. The injury is particularly apparent on all wall-trees, such as the peach, nectarine, apricot, pear, and apple. The buds of the standard fruit-trees, in certain situations, are much injured. Fig-trees are destroyed to within eighteen inches of the ground.

Since the publication of my report on the effects of the frost, on 11th January, the injuries inflicted on the Coniferæ are becoming more and more visible. For a time it was thought that the specimens of *Araucaria imbricata* would get over the browning of their points, which was the first indication of the effects of the frost upon them. The weather for three or four weeks after the frost was very favourable for their recovery. In all low, damp situations the plants are now found to be wholly destroyed, being soft and brown all over, and having the tops and the ends of the branches bent down; while in dry situations, the branches, though more or less brown, are greener and more rigid. About the 4th February the ends of many of the branches were blown off; but, as the branches are hard and firm at the distance of a few inches below the broken points, hopes are entertained that they may yet push out buds. During the frost there oozed out from the stems of all the large Araucarias in the garden a quantity of thick resin. This was not observed on any of the smaller specimens. The largest plant in the garden, now 23½ feet high, with a stem 4 feet 3 inches round, the circumference of the branches being 54 feet, has suffered more than any of the other large specimens. During the last three weeks (February) many specimens of the *Cedrus Deodara* are showing evident signs of great injury—the leaves assuming a grayish-brown hue, and being rapidly thrown off. The old wood is affected, but it is hoped the plants will recover. The *Abies Morinda* in all situations has also become very brown; while, in damp places, all the young shoots are dead, and, in some cases, the entire plant. The branches of all the plants of *Cupressus macrocarpa* seem destroyed. The largest plant is 18 feet high, and 26 feet in circumference of branches, with a stem 3 feet in circumference. By removing the branches, and taking off 6 or 8 feet of its stem, hopes are entertained that it will yet sprout from below. *Cupressus torulosa*, 10 feet high, is also killed. *Cupressus Lambertiana* and *C. sempervirens*, also of large size, seem more or less injured. Large old plants of *Pinus Pinea* are totally destroyed, both on high and low parts of the garden, the largest specimen being 25 feet in height, and having a stem 4 feet in circumference. *Pinus insignis*, *P. radiata*, and *P. muricata*, are all alike brown.

A peculiarity observed during the frost was the splitting of the bark and wood of several shrubs and trees. In the case of the Portugal laurels, several had rents fully half an inch wide, extending 8 or 10 feet along the stem. When the thaw took place the fissures instantly closed. The same splitting was observed in the American lime (*Tilia americana*), sweet chestnut (*Castania vesca*), walnut (*Juglans regia*), sycamore (*Acer Pseudo-platanus*), hornbean (*Carpinus Betulus*). In the case of one of the walnut trees there had evidently been a split during a previous year, the edges of which had adhered only by the bark, without any union by the wood. In several instances after the thaw took place the fissures became so completely obliterated that it was almost impossible to tell where they had been. This splitting has again appeared in some of the trees, especially in the stem of the *Acer Pseudo-platanus*. This may proceed from the swelling of the wood caused by the recent ascent of the sap.

The splittings in the Botanic Garden trees took place on different sides of the stems, as follows :—

Chestnut . .	showed splits on	S.E. side.
Walnut . .	,,	S.W. ,,
Carpinus . .	,,	S.W., N.W., and N.E.
Sycamore (2 trees)	,,	S.W.
American lime .	,,	S.W. and N.E.

This frost-splitting has been particularly noticed by Dr Robert Caspary, in a memoir which he published some years ago. He says that in cold winters some hardy trees, both indigenous and exotic, are often split lengthwise, even to the pith, by frost. In January and February 1855, on the coming on of severe weather, fissures were seen in many trees in and near Berlin. The meteorological relations at that time were as follow :—The commencement and middle of winter 1854–55, till 13th January, was, with the exception of a short cold period in November, unusually rainy. The mean temperature of November 1854 was about 26° Fahr. The thermometer was mostly above the freezing-point, although the absolute minimum on the 15th of the month was 16° Fahr. In December the mean temperature was 37° Fahr.; and the absolute minimum, on 21st of December, was 26½° Fahr. In January 1855, the lowest temperature, which occurred before the 13th, was 30° Fahr. on the 10th. From 13th January to 14th February, a more or less severe frost prevailed, which was only interrupted between the 4th and 7th February by a milder temperature, and partly by rain, which froze on the ground. But between the 4th and 7th February the thermometer never reached 34½° Fahr.; and even during the rain it was generally below the freezing-point, so that a real thaw never took place between the 13th January and 24th February. On 2d February, Dr Caspary noticed the reopening of previous frost-clefts on numerous lime-trees. They seem to have taken place when the thermometer indicated a minimum of −2° Fahr. The clefts continued open during the frost. Fresh splittings were also seen in a lime-tree and a horse-chestnut; the latter split with a loud report. Clefts were also seen in *Quercus pedunculata*, *Q. sessiliflora*, bird-cherry and ash. Dr Caspary found the frost-clefts common in trees from 7 to 20 inches in diameter. They were rarer in stems of less diameter. The splittings always occurred at a time of intense frost. When the horse-chestnut burst, the temperature was −6° Fahr. The splittings generally took place during night. It seems probable that the splitting is caused by a freezing of the sap, which causes expansion under the bark. In order that this freezing may take place, a low temperature is requisite. As the sap contains salts and organic matter, it does not freeze at 32°. Hunter (Philos. Trans., 65, 2, p. 447) found that the expressed juice of cabbage and spinach freezes at 29° Fahr. The sap of living plants freezes in flowers and young shoots at a few degrees below 32°. Goeppert states that from 4½° to 9° Fahr. of frost, continued for a few days, will cause the sap of all trees, not excepting conifers, to congeal. It seems probable that before splitting the trees were frozen to their centre. Frost-splitting gives an excellent opportunity of observing the twisting of the stems of trees noticed by Alex. Braun.—(*Report, Berlin Acad.*, August 1854).

In the severe winter of 1683–84, the phenomena of tree-splitting was remarked. Mr Jacob Bobart, in his report published in the "Philosophical Transactions," says,—"In this rigid season, nothing seemed more surprising to us than the cleaving or splitting of trees,—as of the elm, ash, walnut, and oak. It hath been most frequent among oaks, many of which have been divided to great detriment in England, some being so rent that a man may see through them, and that many times the cracks came with so great noise, that, as it is related from Needwood Forest, they made such a noise, that the keepers there thought that the deer were shot by the people of the country, and that in several parts they were heard as loud as guns. These rifts or clefts were not at all to the same point of the compass, but sometimes on one side only, sometimes two, sometimes three, and sometimes four several places, dividing or quartering the tree, and sometimes quite through; and these clefts were not only in the bodies, but continued into the larger boughs and limbs of the tree, and sometimes descended into the superficial roots, but not to those very deep in the earth. A great part of the cause of it is supposed to be imperfection in such a tree."

A similar remark was also made by Caspary, who says, that in the frost-clefts of thirty-three trees examined, he found some local injury which had weakened the cohesion of the wood tissue. In some cases there was decay from the rotting away of a branch, from a stump not being covered or overgrown, or from the bruising of the bark and young wood. In other cases there was weakening by a slit in the bark previously made. Bobart in his paper refers to the expansion of the frozen sap as the cause of the splitting. He states, "It need not prove troublesome to any to think the ice to be able to tear the oaks and other trees, who shall consider the great force and elastic power thereof." Bobart also says, "We see the fissures close quickly after the frost, insomuch that they are scarcely discernible, and the bark not being divided from the stem, upon coming together again, each turn and twist of the grain fitting its place, prove fresh and vigorously growing. But that ever such trees will prove whole and sound doth scarcely consist with reason or our present thoughts. And the calamity hath not been found in trees only that were fresh and standing, but also in trees cut down; but notwithstanding, it is thought to be only among such diseased trees as are before mentioned." A similar fact as to felled trees was mentioned to me by Mr Robertson of Trinity Nurseries. An ash which had been cut down, when touched by a saw during the late intense frost, suddenly split with a loud noise.

Bobart also refers to the effects of the sun on plants with a southern exposure, and shows the injury caused in such cases by frosts coming on at night. The difference also of the temperature at which various kinds of sap freezes is noticed. "Trees all the winter," he says, "while the sap remains condensed [that is not running], are safe and well; but if a flattering too early glance happens in the spring to set their parts in action, and the juices to become fluid, and a sudden mutation of that warmth to a fresh return of winter, which too frequently happens in England, then we have our hopes of that year's fruit blasted. Hence is supposed the decay of the Glastonbury thorn, whose arising time being between Michaelmas and Christmas, being sappily prepared by the beginning of the hard frost, which hath almost affrighted it out of its life."

He supposes that the presence of essential oil and other substances in the wood of trees may prevent them from being much injured by frost. To this he refers the endurance of cold by firs and pines in Norway and other cold countries.

In the severe year of 1683–84, it appears that yew and holly were much injured, and in some places killed; the furze was also killed or cut down to the ground. Common broom proved a degree hardier. "It is hard," Bobart says, "to say what is winter proof, even among our natives, except box and ivy, which stand in defiance of all." In the gardens many evergreens suffered, as alaternus, phillyrea, common bays and laurels, many of which were completely destroyed, others severely injured. Rosemary, laurustinus, halimus (sea-purslane), arbutus, white jasmine, and other plants which seldom fail, were generally killed throughout the whole country. "But in all these, and such like, in mountainous and dry places, there is a brisk life and verdure yet remaining, enough to retain the several species among us. Amongst deciduous trees divers have been sufferers, as the Judas tree [*Cercis Siliquastrum*] and plane-trees; *Paliurus* [*aculeatus*], the Aleppo ash [*Fraxinus lentiscifolia*], and the Locust tree [*Robinia*]. The common bramble has also been injured."

He mentions that great destruction had taken place in the case of artichokes, winter coleflowers, sage, thyme, mastick, lavender, lavender-cotton. He also alludes to the protection given by snow to many herbaceous plants, and the injury done to grain crops in cases where, by a temporary melting of the snow, the plant had been exposed to a subsequent frost.

He concludes, that after all this repetition of sorrows "we are to comfort ourselves that such destruction and calamity happens but rarely, the like having not been known in the memory of man, if ever before, and that with due care and observance the growing cold might be kept off from such things as are proved to be impatient of it; which are not the greens [evergreens] in our gardens, some being able to endure all the cold that ever comes, as firs, pines of divers sorts, cedars of Lebanon and Virginia (though that of Bermuda proves tender), arbor-vitæ, all the savins, whereof the upright or berry-bearing is the best succedaneum to [substitute for] cypress, capable of finer cutting into pyramids and other figures, or hedges 6 or 8 feet high, and is one of the best of the tonsile shrubs; also the *Pyracantha* proves exceeding hardy, and makes good hedges."

Mr J. Evelyn also notices the effects of the frost of 1683-84. He mentions the destruction of cork trees, cedars, alaternus, rosemary, halimus or sea-purslane, of which he had a hedge, and cypresses. No injury was done in his district to ilex, scarlet oak, arbutus, bays, laurel, laurustinus, or hollies.

The winter of 1837-38 was unusually severe, and the temperature occasionally very low. The injury done to trees and shrubs during that season will long be remembered. All the large plants of *Arbutus Unedo* and *A. Andrachne*, in the Botanic Garden, with numerous specimens of Laurustinus, and other shrubs, were destroyed. Mr M'Nab states that the great destruction which took place that winter may be attributed to the following circumstance. After a black frost, the ground was covered to the depth of five or six inches with snow, which afterwards became hardened by frost. After this, a sudden thaw took place, which caused the snow to melt, but did not act on the frozen soil. On this account the stems of the plants were surrounded by pools of water, which could not be carried off by the hardened soil. During the night of 20th January the thermometer fell to 6½° Fahr. This caused the water to become suddenly frozen, in consequence of which the bark round the lower part of the stems was destroyed, and the death of the plants took place. On the 30th of January 1845, the thermometer fell to 4½°. The day following, the sky was clear and the sun unclouded, and the loss of evergreens was very great.

During last autumn, the shrubs were in a remarkable growing condition. This was owing to excessive moisture. The soil in the Botanic Garden, for the most part, is of a very dry, sandy nature. Throughout the autumn months many large evergreen shrubs were transplanted, and the dry unmoistened soil was found rather troublesome to keep together. It was only in the case of those transplanted during November that the balls of earth were sufficiently moist to adhere together. This autumn moisture caused a late growth, which was particularly apparent in many of the young shoots of the hardier plants, as hollies and other evergreen shrubs, and on these the frost seems to have acted most severely. This late growth was very marked in all the Araucarias, which may account for the great injury sustained by them. Amongst Deodars, a few seem slightly altered in colour, but as yet nothing serious is apprehended. During none of the frosts of previous years did the plants of *Aucuba japonica* show the same amount of blackened leaves as they have done since the frost of 23d December.

Extracts from Letters to Mr M'Nab and Dr Balfour relative to Temperature and to the Injury done to Vegetation during the past winter.

1. *From* Mr James Stratton, *Botanic Garden, Cambridge.*

28th January 1861.

Evergreens, such as Cherry Laurels and Portugal Laurels are all but killed. Oaks of the *Ilex* division are still more injured, Phillyreas, Alaternus, Arbutus, Sweet Bay, are dead to the roots. Several Coniferæ, as *Pinus Sabiniana, P. Brutia, Araucaria imbricata, Cedrus Deodara*, and even the common Cedar of Lebanon, are injured—some individuals more than others.

Your temperature and ours seem to have equalled each other, but on different days. On the 26th of December I was not alarmed enough to note it particularly.

Morning of December 25, 1860,	.	5° below zero.
„ January 8, 1861,	.	5° above zero.
„ „ 10, „	.	10° „

We had about six inches of snow on the ground, which, I trust, has saved many of the herbaceous plants that would otherwise have perished.

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2. *From* Mr James C. Niven, *Botanic Garden, Hull.*

23d January 1861.

You will see by the tables sent, that we have not had the severity of frost which you appear to have experienced at Edinburgh. Our thermometer was placed in a favourable situation for a fair registry, viz. two feet from the ground, and not covered. Our Portugal Laurels, common or Cherry Laurels, Phillyreas, Arbutus, Rhamnus, Sweet Bay, Laurustinus, appear to be all killed as far down as the snow-line. Even Hollies, more especially the variety *Ilex crassifolium* (leather-leaved), and several variegated varieties, will lose every leaf. All our Araucarias, so far as I can judge, are seriously damaged, if not absolutely killed. So are *Cupressus macrocarpa*, *C. funebris*, and *Cryptomeria japonica*. *Pæonia Moutan* is killed to the ground; also *Aucuba japonica*, and many of the supposed hardy Rhododendrons. The chief reason why we have suffered so much, is, in my opinion, the large amount of rain we had during summer, which caused a vigorous growth among all evergreens, without warmness or dryness in autumn to ripen the wood. Our greatest depth of snow did not exceed 8 inches. On the 29th of December, while the thermometer was at its minimum, we had a dense mist (a frozen one of course), but with all the appearance of fog, which continued for two hours, after which the thermometer gradually rose.

Minimum Temperature at the Hull Botanic Garden.

	Degs.		Degs.
1860, December 23 .	22	1860, December 28 .	22
„ 24 .	15	„ 29 .	8
„ 25 .	0	„ 30 .	23
„ 26 .	10	„ 31 .	25
„ 27 .	22	1861, January 8 .	10

Particular Observations on the 26th and 25th December.

	Degs.		Degs.
December 24, 3 o'clock P.M.	22	December 25, 2 o'clock A.M.	3
„ 5 „	16	„ 4 „	3
„ 7·30 „	6	„ 7 „	zero.
„ 10 „	4	„ 9 „	2
„ 12 „	6	„ 11 „	6

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3. *From* Mr DAVID MOORE, *Glasnevin Botanic Garden, Dublin.*

22d January 1861.

I herewith send you the readings of our thermometer during the week of the intense frost which has proved so fatal to plants in England and Scotland. We have not suffered so much as on three former occasions of frost, which occurred within the last twenty-three years. Our Evergreen Oaks are as green as possible, and none of the Coniferæ are killed which usually escape. The specimens of Laurustinus are, as usual, considerably injured, particularly the shining-leaved variety, while the black, rough-leaved variety is perfectly safe, and now beginning to expand its flowers. From what I have stated, you will be able to judge how little we have suffered compared to you and our English neighbours. Araucarias look always frosted in the County Dublin, so that they are no criterion to judge from; but they have not fared worse than they do in ordinary winters. On the 19th and 20th of December we had snow, which covered the ground from 6 to 8 inches; it remained on the plants when the intense frost took place, and was there frozen. To this circumstance I attribute the comparative safety of many of our plants, since we have not suffered nearly so much as we did four years ago, when the lowest point registered was 9°.

Minimum Temperatures at the Glasnevin Botanic Garden.

	Degs.		Degs.
December 21	22	December 25	24
„ 22	18	„ 26	29
„ 23	16	„ 27	32
„ 24	8		

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4. *From* Mr John Bain, *College Botanic Garden, Dublin.*

1st February 1861.

Although our shrubs, &c., have been greatly injured by frost, we have not had anything like the intense cold you have experienced. The following plants have been killed, at least to the surface of the ground :—Shining-leaved Laurustinus, all kinds of Ceanothus, except *C. dentatus*, standard *Magnolia grandiflora*, Aristolochias, Edwardsias, Myrtles, Phillyreas, Alaternus, and some hybrid Rhododendrons. Those very much disfigured are Magnolias on walls. *Ilex furcata* and *cornuta*, Evergreen Oaks, black and common Laurustinus, Sweet Bays, and common Arbutus. Sunday the 23d of December was the coldest night we had, the thermometer being 24° below the freezing point, that is 8° Fahr. In the spring of 1855 we had the thermometer 29° below the freezing point, *i.e.* 3° Fahr. or 5° lower than the 23d of December last.

Minimum Temperatures.

	Degs.		Degs.
1860, December 22	21	1860, December 25	24
„ 23	8	„ 26	34
„ 24	14	1861, January 8	39

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5. *From* Mr Daniel Ferguson, *Royal Botanic Garden, Belfast.*

21st January 1861.

I send you the readings of our thermometer, by which you will see that we have not had the same amount of frost as you have experienced at Edinburgh, indeed in December 1859 we had it lower by 7°.

Minimum Temperatures.

	Degs.		Degs.
Morning of 23d December	18	Morning of 26th December	32
„ 24 „	11	„ 8th January	17
„ 25 „	11		

On the morning of the 24th December the thermometer at Lurgan was down at 9°, which is the lowest that I have heard of in the north of Ireland.

We have escaped with comparatively little injury. Any plants which withstood the frost of last winter have also withstood this winter. However, all our Laurustinus, and nearly all our Roses, are completely killed. This can be accounted for from the wet autumn, the wood not being well ripened.

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6. *From* Mr A. Anderson, *Oxenford Castle Gardens.*

22d January 1861.

I send you a list of our injuries from the late frost. I will be better able some months hence to give a correct statement. I have been looking over our fruit-trees, and find the buds of many of our hardiest fruit-bearing trees are killed. And I fear that our crops will not be very abundant this season. The wood and buds were not well matured last autumn, consequently were ill prepared for such a winter.

Apple, Pear, Plum, and Apricot buds are injured. Standard Roses are killed (all sorts). The Cotoneasters, on walls both with south and north aspects, are injured. The leaves of the Gold and Silver Hollies are blackened. The common green Holly is blackened in some places. Portugal Laurels are injured in some situations, inside leaves killed, outside leaves safe, and where autumn pruning had been practised, they are much injured. Araucaria, last year's growth, killed. Laurustinus killed above snow. *Cedrus Deodara* generally safe; one plant (10 feet high) a good deal browned. Taxodium (14 feet) safe. Do. (7 feet) injured. Do. (5 feet) underwood, safe. Rosemary (10 feet high) killed on a south wall. Evergreen Oaks killed. A curious effect appears among Portugal Laurels, one bush may be seen as green and fresh as at midsummer, and those on each side very much hurt. I have observed the same thing occur among early potatoes in spring on a frosty night, when one would remain, while all around were killed; showing that a very small obstruction will turn aside the cold current, or prevent radiation, in some way.

Readings of Thermometer for one week, at Oxenford Castle Gardens, 450 *feet above the sea-level.*

	Degs.	
Sunday Night, Dec. 23,	5	above zero.
Monday ,, ,, 24,	6	,,
Tuesday ,, ,, 25,	4	,,
Wednesday ,, 26,	7	,,
Thursday ,, ,, 27,	6	,,
Friday ,, ,, 28,	15	,,
Saturday ,, ,, 29,	27	,,

Our coldest night during this severe weather was Monday, 7th January 1861. Thermometer, five feet from the ground, was 2° above zero; and on the surface of the snow, 1° below zero.

7. *From* Mr Wm. Baxter.

Riccarton, 21*st January* 1861.

Our Coniferæ, upon the whole, have not suffered much; but I fear our fruit-trees, such as Peaches, Nectarines, and Pears, are very much injured—to the extent, I should say, that no fruit may be looked for the ensuing season, more especially as regards Peaches. Our Araucarias are all more or less browned at the points of the shoots, which are also drooping. *Cupressus torulosa* has suffered a good deal, as also *Libocedrus chilensis*. On the whole, the Californian species are not much hurt. On some low ground, a few Deodars are almost, if not altogether, killed; and I may say as much of all the spring-planted Portugal Laurels. Some planted about midsummer escaped.

We had considerable variations in our thermometers about this neighbourhood on the morning of 24th December 1860. Our garden thermometer fell to 7°; at Riccarton House, 4°; at Hermiston House, Miss Craig's thermometer indicated 3°, and the gardener's 1° below zero; Mr Dickson's, 5° below zero; Gogar Bank, 3° above zero; Hanley, 8° below zero; and Currie Hill, 4° below zero. The average of the above figures would, I think, give the probable temperature of this district.

8. *From* W. Scott Elliott, Esq., *to* Professor Balfour.

32 Regent Terrace, 26*th January* 1861.

The following is the range of the thermometer during the late storm, kept at Kilgraston, Strathearn, Perthshire. The exposure was at the dining-room window, facing the north:—

Dec. 22,	10° above zero.	Dec. 26,	3° below zero.
„ 23,	15 „	„ 27,	1 above zero.
„ 24,	2 below zero.	„ 28,	1 „
„ 25,	0		

9. *From* Humphrey Graham, Esq., *to* Professor Balfour.

11 Shandwick Place, Edinburgh, 30*th Jan.* 1861.

I send you a note of the thermometer at my pinetum or arboretum during the late storm. My trees, both young and old, deciduous and evergreen, are all injured by the weather; but starving hares and rabbits have made sad havoc chiefly among Laburnums and Hollies.

Copy from register-thermometer kept by Alexander Donald, gardener, at Bilstane, about 850 feet, by Ordnance map, above sea-level, parish of Kirknewton. Thermometers (Fahrenheit) are kept in a wooden box about four feet above the ground, covered at top, open only to the north:—

Minimum Temperatures.

Dec. 22, . . .	13°	Dec. 26, . . .	6
„ 23, . . .	16	„ 27, . . .	11
„ 24, . . .	4	„ 28, . . .	11
„ 25, . . .	12	„ 29, . . .	24

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10. *From* Mr JAMES STEWART, *Cairnsmuir, Wigtonshire.*

31st January 1861.

In answer to your inquiries, I am happy to say that none of my Araucarias seem to have suffered from the severity of the frost. I was from home the week when the thermometer was at its lowest, and I do not think a correct register was kept of it. However, I am satisfied that we had not such severe frost here as must have prevailed in some parts of the country much further south and west of us. As a proof of this, I may mention that the *Cupressus funebris* has stood the winter in the garden here without any protection; and I consider it rather a tender species. The *Cryptomeria japonica* and *Taxodium sempervirens* have also braved the winter. I cannot say so much for Roses; some of the *leaves* are blackened, but I hope the *shoots* have escaped in general.

I have long noticed that the perfect ripening of the young wood on trees and shrubs in autumn is the important point for enabling the plants to meet severe frost without damage; and I think, in this respect, in our neighbourhood, we had some advantage last autumn.

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11. *From* Mr WM. STEWART, *Cockpen House, Mid-Lothian.*

31st January 1861.

On looking particularly at our fine evergreen Oaks at Dalhousie Castle, I find they have shared the same fate as those in the Botanic Garden. There are a good many of the branches with the leaves quite green, but the main trunk of the tree seems gone. The damage done to shrubs will not be fully known until the spring tries them.

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12. *Report by* Mr PETER LIDDLE, gardener to Sir James Matheson, *on the Flowering of Plants at Stornoway, dated 4th February* 1861.

Anemones were in flower at Stornoway on 1st January 1861, Primroses on 16th, Snowdrops on 20th, and *Petasites alba* on the 25th. *Cydonia japonica,* Alders and Willows, are now in flower (4th February), and Crocuses are beginning to show flowers. The flower-buds of *Ribes sanguineum,* of the Elder, of the Mezereon, and of *Berberis* (*Mahonia*) *Aquifolium,* are beginning to expand. There is a marked swelling of the buds of the Mountain Ash, Horse Chestnut, Wild Cherry, Sycamore, Broom, Whin, Thorn, Larch, Lilac, and Laburnum. The young shoots of *Leycesteria formosa* were about half an inch long when the frost set in, but they were not injured. Some standard Camellias, 3½ feet high, in a shrubbery border, in a very exposed place, which had their roots covered with a few pieces of spruce, and their stem surrounded with a hay-rope, have scarcely suffered from the frost. None of the Coniferæ have suffered, with the exception of a species of Juniper. Most of the Rhododendrons made a second growth in autumn, and yet they have only been slightly injured in some exposed situations. *Escallonia macrantha* has suffered to a certain extent. Loniceras, Fuchsias, and Hollies, have not been injured. Celery and Brocoli have been supplied from the garden all winter; they have had no protection. The grass never lost its green appearance, and daisies have continued to flower since 20th November.

Specimens of many of the plants mentioned have been sent to Edinburgh. Among these were *Cydonia japonica* in flower; *Petasites alba* in flower; Willow, Hazel, Alder, and Beech, with catkins more or less developed; *Viola tricolor*, var. *arvensis* in flower; Snowdrops, *Anemone coronaria*, and *Primula vulgaris*, in flower; *Mahonia* bearing flower-buds; *Leycesteria formosa* and *Rhododendron ponticum* with vigorous buds.

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13. Mr Thomas Robertson, gardener, of Duffus House, Banffshire, wrote thus on 11*th February* 1861:—"All the China Roses and the Fuchsias are cut down to the ground. The evergreens, however, appear to be safe. The buds of Apples, Pears, *Ribes sanguineum*, and Gooseberries, have been injured much."

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14. Dr John G. Innes, writing from Forres, 11*th February* 1861, says—"I send herewith the readings of the maximum and minimum thermometers for the last nine days of December and 1st day of January:—

	Max. in shade.	Min.	Max. in sun.
1860. Dec. 23,	34·1	16·0	35·5
„ 24,	27·1	9·0	31·4
„ 25,	28·4	13·5	30·0
„ 26,	30·0	13·0	32·0
„ 27,	31·8	22·3	32·4
„ 28,	31·0	15·0	32·0
„ 29,	33·5	27·0	36·2
„ 30,	37·0	30·0	38·5
„ 31,	39·0	36·0	45·0
1861. Jan. 1,	38·0	33·0	39·5

"I went to Dalvey to-day, and found that the frost had done very little injury to the plants there. Deodars quite sound; *Crypto-*

meria japonica, ditto. Laurels and Arbutus a little browned in the leaves, but apparently not otherwise damaged. Yellow Jasmine in full bloom. Fruit buds few in number, but sound when cut into.

Roses here have suffered severely in consequence of the young wood not being sufficiently ripened last autumn—Noisettes and Teas, and even hybrid Perpetuals, requiring to be cut close in.

I enclose a shoot of Deodar, a bit of Cryptomeria [apparently *C. Lobbii*], a twig of the old Hawthorn, yellow Jasmine, and one or two Primroses, as a sample of our vegetation at this season.

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18. *From* Mr D. CUNNINGHAM, *Palace Gardens, Fulham.* *9th February* 1860.

The thermometer, at 4 feet from the ground, on the 25th December 1860, stood at 7 degrees. Tea Roses, Euonymus, and young plants of *Cedrus Deodara*, have been killed. *Laurus nobilis*, Myrtles, Magnolias, and Pampas grass, have been very much injured; an old cork tree, and old Deodars, have also suffered; Araucarias, Cryptomeria, and Wellingtonia, are uninjured. None of the forest trees have suffered. But in Essex, Herts, and Derbyshire, where the thermometer reached zero, Ash, Yew, and other forest trees have been split.

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15. *Report from* Mr PETER BOYS, *Jardine Hall Garden, Dumfriesshire, 5th March* 1861.

Thermometer on morning of 24th December 1860, at 7 A.M., was five degrees below zero; the exposure being due north.

The following plants appear to be killed by the frost:—*Cryptomeria japonica, Cupressus M'Nabiana, Taxodium sempervirens, Gynerium argenteum* (Pampas grass), *Weigela rosea, Abies Mertensiana* (most of them killed), Brocoli, Greens, and Savoys.

The following are injured more or less severely:—*Araucaria imbricata,* branches below killed, upper ones browned, ends of branches drooping; *Aucuba japonica,* leaves and branches black; China Roses, Perpetual Roses, *Erica stricta* and *herbacea, Dabœcia polifolia,* Bay and Portugal Laurels, Common Yew and Ivy, in exposed situations, have their leaves browned. Laurustinus and Arbutus have never thriven well at Jardine Hall. Plants of *Cedrus Deodara* have their leaves browned in some instances.

The following plants seem to be uninjured:—*Pinus Jeffreyi, Abies Douglasii, A. cephalonica, A. Pattoniana, Picea grandis, P. Nordmanniana, Wellingtonia gigantea, Thuja gigantea, Cupressus Lawsoniana, Juniperus virginiana, J. macrocarpa, Cedrus Libani,* Rhododendrons, Kalmias, Andromedas, Berberis, Ribes, Jasminum, Lonicera, Cotoneaster, Box and Holly, *Salisburia* *adiantifolia*, *Deutzia scabra*, *Forsythia viridissima*, *Spiræa Lindleyana*, *Pæonia Moutan*, *Ledum buxifolium*, *Hibiscus syriacus*, *Kerria japonica*, *Nuttallia cerasiformis*, *Aristolochia Sipho*, *Cydonia japonica*, Mistleto, Tulip-tree, Magnolias, Chestnuts, Plum, Apricot and Peach, Raspberry, Strawberry, Asparagus.

16. *Report from Culloden, Inverness-shire.*

Temperatures.

Dec. 23.	+22°·3	Dec. 26.	+11°·7
„ 24.	+11 ·2	„ 27.	+24 ·8
„ 25.	+21 ·7		

The following is a list of plants, shrubs, and trees, more or less injured (so far as is yet known) by the frost, in the garden and pleasure-grounds at Culloden: *Escallonia rubra*, killed; *Erica vagans*, much injured; China Roses, Tea-scented and Bourbon, almost killed; *Phillyreas*, all nearly killed; *Laurus nobilis* (Sweet Bay), killed to the ground; *Aucuba japonica*, much injured; *Arbutus Unedo*, killed; Laurustinus killed to the ground; Common Laurel (Bay and Portugal) slightly touched in some places. Some Peach-trees slightly touched, but Apricots safe.

17. *Report from Thirlestane Castle, Berwickshire.*

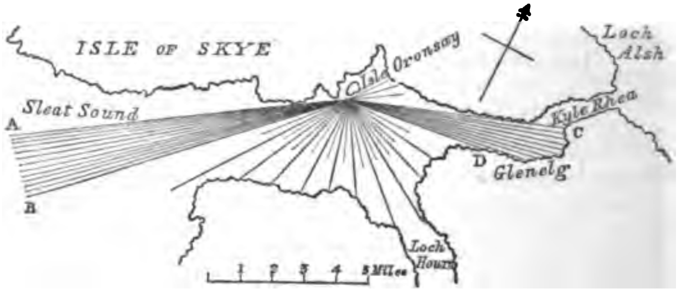
Temperatures.

Dec. 23.	−4°·7	Dec. 26.	−0°·7
„ 24.	−6 ·7	„ 27.	+4 ·3
„ 25.	−8 ·7	„ 28.	+2 ·3

Roses, Auracarias, Aucubas, Escallonias, Leycesterias, and Common Laurels, are all killed down to where they were covered with snow. Portugal Laurels, Common Ivy, Privet, *Cedrus Deodara*, the last year's wood of Pears, Plums, and several sorts of Apples, very much destroyed.

On Azimuthal Condensing Apparatus of Unequal Power adapted for Fixed and Revolving Lighthouses placed on Islands near the Shore. By THOMAS STEVENSON, F.R.S.E., Civil Engineer.

In the volume of this Journal for 1855, I described different methods of distributing the rays proceeding from a flame equally over any required azimuthal angle. These devices are now applied to such fixed lights as are situated on the shores of Sounds or narrow seas, where the power of the light ought to vary according to the varying width of the channel to be illuminated. In fixed lights, as formerly constructed, the portion of the light which was next the shore was either allowed to be wholly lost, from its showing in the landward direction, or it was returned through the focus by means of a sector of a spherical mirror, so as ultimately to strengthen a corresponding arc of the sea horizon. Cases, however, very frequently occur, in which it is desirable to employ this spare light, not in the direction exactly opposite to the dark arc, but in some other direction better suited to the configuration of the coast line. For example, at Isle Oronsay, in the narrow Sound of Skye —which was one of the first places where the condensing apparatus was adopted—the light throughout nearly the whole of the illuminated arc does not require to be seen at a greater distance than three or four miles; while in one direction down the Sound, towards A B, it may be seen from a vessel's mast for about fifteen miles; and in another up the Sound, towards C D, it should be seen for about seven miles. By means of condensing apparatus similar to that which was first described in this Journal, the spare light is condensed so as to increase the power up and down the Sound, and in this way one small lamp and an apparatus of small size are found sufficient for the purpose. The practical effect thus secured is, that a lamp consuming only about 210 gallons of oil annually produces a beam of rays (in the only direction in which great power is required) equal in effect to a first-class light consuming about 750 gallons. Condensing lights have now been established at Kyleakin and Isle Oronsay, both in the Sound of Skye;



Phladda Island near Easdale, Runa Gall in the Sound of Mull, Corran Point in Loch Linnhe, and another is now being fitted up at MacArthur's Head, in the Sound of Islay. It has been computed, that the *saving* in first cost and consumption of oil, consequent on adopting this improvement at these six lighthouses, amounts, on the most moderate estimate, to about L.385 annually.

Since the first application of this principle of *allocating the light to the different azimuthal arcs in the compound ratio due to their lengths of range, and the number of their degrees in azimuth,* various improvements and extensions of the same principle have occurred to me; and I now, as a supplement to my first communication, add the following cases, both for revolving lights, and for fixed lights which require to show all round the horizon:—

Revolving Lights of Unequal Range which do not Illuminate the whole Horizon.

Revolving apparatus, where the whole horizon does not need to be illuminated, may, like the fixed apparatus, be similarly condensed. The power may be increased in the required directions in different ways, according to the size of the apparatus and the local requirements of the coast line. As, for example, at the Skervuile Rock, in the Sound of Jura, (where I first thought of applying the condensing principle to revolving lights), the method that seemed most suitable was to have the vertically straight prisms or mirrors for diverting the light into the required arc (*vide* vol. i. p. 273) fixed outside of the revolving apparatus. By this arrangement, whenever the revolving lenses passed out of the illuminated arc into the dark arc, the beams of rays emanating from them would be reflected by the series of prisms or mirrors, so as to pass over the arc that needed strengthening, which would thus be illuminated at the same instant by a direct and a reflected beam. The erection of a light on Skervuile was, however, delayed, and still remains in abeyance, and nothing was done regarding this plan of condensing. At MacArthur's Head, which was at one time (1859) intended to revolve, the apparatus was so large that the lantern would not admit of reflectors being fixed outside of the apparatus, as proposed for Skervuile, and the mirrors were therefore to have revolved along with the lenses, &c.* Attached to each mirror was a rod or standard, projecting downwards, with a roller on its lower end, which in its transit over the dark arc passed up an inclined plane fixed to the trimming-path. In passing over the inclined plane the rollers would of course rise upwards, and raise the mirrors to the level of the lenses, so as to intercept the beam of light, and reflect it parallel to a beam from one of the other faces of the revolving apparatus, and thus direct it to the azimuth where additional strength was wanted. At the other extremity of the dark arc, the rollers were to be lowered by descending another inclined plane, and the mirrors descending with the rollers below the level of the lens, became again inoperative. After the drawings had been nearly completed, the character of the light was altered from a revolving to a fixed light, so that the plan has never yet been carried into practice. Other mechanical arrangements may, however, be found preferable in some cases.

Fixed Lights of Unequal Range, which Illuminate the whole Horizon.

At fixed light stations, which require to show *all round* the horizon, but which are placed, for example, on an island very near the shore—it becomes desirable to allocate the power in proportion to the lengths of range in the different azimuths. The light should for those azimuths (which require to be more or less powerful than the rest) be collected into beams of parallel rays by sectors of holophotes proportionate in horizontal angle to the power required. There should then be placed in front concave divergers, straight vertically, and of such horizontal curvature as to spread the incident parallel rays over the required arcs. The light will thus be made to show all round the horizon, of power *equal* to a fixed light of the ordinary kind, in those azimuths where the ordinary fixed light apparatus is placed, and of different powers in the azimuths in which the holophotes and divergers are placed. For by this arrangement, the light will be reduced in the azimuthal plane *below* the standard of the light from the ordinary fixed apparatus, or increased *above* that standard in proportion to the allocation and separation of the rays effected by the compound action of the holophotes and the divergers. One of these agents could generally be saved in the refracting part of the apparatus, by making it disperse or condense horizontally, while still continuing to parallelize vertically, though it might perhaps be impracticable to make such a change in the totally reflecting prisms, the execution of which is already difficult enough. This, however, is of no consequence, as it would in all cases be sufficient to restrict the condensation and expansion, to those rays which pass through the refracting part of the apparatus only; for even the shortest range would probably need all the light which comes from the prisms.

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* My friend, Mr James Balfour, C.E., has lately suggested to me, that where the apparatus is of small size, one continuous mirror might be employed instead of a series of separate mirrors. This mirror should of course be a logarithmic spiral.

Revolving Lights of Unequal Range, which Illuminate the whole Horizon.

When a *revolving* light is placed, for example, on an island situated in a long and narrow sound, and is required to *show all round the horizon*, it is obvious that, as in the case of the fixed light in similar circumstances, it should not be equally powerful in every direction. The simple method of distributing the rays properly, in many cases, is to place between the lamp and the revolving apparatus zones of spherical mirrors (whose centre of curvature is in the flame), or by placing outside of the apparatus portions of straight prisms, or mirrors of such horizontal curvatures as may be necessary for the different localities, or by a combination of these agents. The vertical breadths of these internal or external agents will depend on the lengths of range in the various azimuths, so as to divert that portion only of the light which can properly be spared from one azimuth to assist another, while they allow the remainder of the rays to continue in their original direction. The varying breadths of these agents which intercept a part of the light in its ordinary course, but which still permit a limited portion to pass above and below them, will therefore represent inversely the varying distances of the neighbouring shores from the lighthouse. This ratio, which is probably inversely as the *squares* of the distances from the shore, would require to be ascertained by a series of experiments.

The allocation might also in some cases be effected by a modification of the method already described for fixed lights, which show all round the horizon. To attain this there would be placed between the lamp and the revolving lenses agents which expand or condense the rays as required over the different arcs. But as the rays so altered in direction would not fall properly upon the lenses, an additional agent* would need to be placed close to the lenses, in order to give the expanded and condensed rays the same amount of divergence as if they had proceeded straight from the centre of the flame, which is the focus for which the lenses are calculated. For this purpose, supposing that we are dealing with an arc that has to be *weakened*, from its being opposite to a part of the coast-line which is *near* the lighthouse, the agent placed next the flame would cause the incident rays to proceed as from a virtual focus short of the lamp, or, in other words, would increase their divergence. The action of this instrument would therefore be to expand the rays incident upon it, and which came from but a small horizontal portion of the flame, and to spread them over the whole of the larger arc, where a small amount of power is sufficient. The second agent, or that next the revolving lenses, would intercept these expanded rays before they fell upon the revolving lenses, and would reduce their divergence, so as to make them proceed as from a virtual focus situated in the centre of the flame, so that they would then be incident on the revolving apparatus in the proper directions. In other words, the second agent would undo the action of the first, but not until the rays had been spread equally over the expanded arc, and had extended so far outwards as nearly to reach the lenses. In the case of an arc of condensation, the arrangement would be nearly the converse of the former. In both cases the agents would only require to act in the horizontal plane, the centre of their vertical curvatures being in the centre of the flame.

In some instances spiral mirrors might be arranged close to the flame so as to send the light in the required direction, without so materially altering the direction in which it falls on the lens as to render a second agent necessary.

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* In some few cases a single agent might be sufficient.

Notice of Skulls found at Kertch, in the Crimea.

By DANIEL WILSON, LL.D., Professor of History and English Literature, University College, Toronto.

The stirring events connected with the Russian war, though already superseded in the popular mind by the memorable incidents of more recent warfare, have left behind them at least one beneficial result, in the knowledge we now possess of the geography and history of the Crimea; along with many valuable traces of the diverse occupants of that remarkable peninsula from the earliest glimpses of Greek colonisation along the shores of the Euxine Sea. From the date of the landing of the Anglo-French army in the Crimea in 1854, its geography, its ethnology, and its antiquities, all acquired a new interest; and the half-obliterated remains of its unheeded and long-extinct past were suddenly invested with a significance which stimulated further investigation, and led to literary and archæological disclosures of permanent value. Among its ancient historical sites, which, owing to peculiar circumstances, received a large share of attention, that of Kertch is, on various accounts, the most remarkable. Built on the site where, some 500 years before Christ, the Greek city of Panticapæum was founded, it was the centre of an area rich with memorials of the strangely chequered past, which has seen the same spot successively occupied by Milesian Greeks, Romans, Goths, Huns, Tartars, Genoese, Turks, and Russians. The Russian occupation of the Crimea dates only from a late period in the eighteenth century; but since then, a museum had been formed in the town of Kertch, in which were preserved many historical antiquities of the Crimean Bosphorus; and especially sepulchral relics recovered from the tumuli which abound on the site of the ancient Milesian colony.

Learning from an old fellow-student that he was about to proceed to the Crimea, to join the Army Medical Staff, I wrote to him, drawing his attention to various objects worthy of observation; and in directing his notice to the treasures accumulated in the Museum at Kertch, specially requested him to note for me—should opportunity offer—the characteristics of an ancient Macrocephalic skull preserved there. It is referred to in Captain Jesse's "Notes of Travel in Circassia and the Crimea," where it is said to have been found in the neighbourhood of the Don, though the probabilities suggested by other notices of discovery of Macrocephalic crania rather favour the idea that the Kertch skull was procured in much nearer proximity to the site of its later depository.

It chanced, as is now well known, that in the fortunes of war the town of Kertch fell into the hands of the Anglo-French invaders; and some few of its ancient treasures were preserved and transmitted to the British Museum. By far the greater portion of the Museum collections, however, were barbarously spoiled by the rude soldiery; and among the rest doubtless perished the little-heeded relic of the Macrocephali of the Crimea, first described by Hippocrates five centuries before our era. Blumenbach has figured, in his first Decade, an imperfect compressed skull, received by him from Russia, which he designates as that of an Asiatic Macrocephalus; and in 1843, Rathke communicated to Müller's "*Archiv für Anatomie*," the figure of another artificially compressed skull, also very imperfect, but specially marked by the same depression of the frontal bone. This example is described by the author as having been procured from an ancient burial-place near Kertch, in the Crimea. And in 1849, M. Rathke published a memoir in which he went into the subject more fully; and showed that the vicinity of Kertch had yielded other illustrations of the same remarkable artificially modified crania of the ancient world, corresponding to those of Peru and the tribes of North America bordering on the Pacific. In illustration of the origin of the Crimean Macrocephalic crania, M. Rathke draws attention to the notices of the ancient tribes who derived their name from the singular practice of modifying the shape of the head during infancy. Hippocrates, in his *De Aere, Aquis, et Locis*, speaks of them as a people among whom "those are thought the most noble who have the longest heads." In this respect, the modern American flat-head tribes, as well as the older Peruvians, furnish a remarkable correspondence in the ideas by which all have been actuated. Among the flat-head tribes, the compressed and distorted skull is the symbol of aristocracy, while the slaves of the tribe are rigidly precluded from giving the prized deformity to the heads of their offspring. Other distorted crania found in the neighbourhood of Vienna have been ascribed to the Avars or the Huns of Attila. But these have been made the subject of a curious commentary, singularly illustrative of the essential correspondence between the artificially modified crania of the Old and New World. Dr Von Tschudi, the Swiss traveller, whose works on the Antiquities and Ethnology of Peru have justly attracted attention, published a memoir on one of the Austrian abnormal crania, in the interval between the first and second publications of M. Rathke, in which he maintained the identity of the abnormal Austrian and Peruvian skulls, and traced the origin of the former to the connection between Germany and Peru in the sixteenth century, when both were under the common rule of the Emperor Charles V. At that period, as he assumed, certain artificially-compressed Peruvian crania had been brought over, along with other curious relics of the New World; and, having been thrown aside, they thus turned up, some three centuries afterwards, to baffle the speculations of modern science. Further discoveries, however, have sufficed to dispel this gratuitous assumption; and it is no longer doubted that the remarkable abnormal skulls, both of Kertch and other localities along the shores of the Euxine and in the valley of the Danube, confirm and illustrate the references by Hippocrates, Strabo, Pliny, and other early writers, to an Asiatic people among whom the very same practices prevailed as still form the special characteristics of many of the north-west tribes of America, on the Columbia and Fraser Rivers, and on Vancouver's Island.

Now that special attention is drawn to this subject, it may be presumed that further discoveries of the same kind will be reported from time to time from the ancient cemeteries of the Crimea, and that probably in the cabinets of St Petersburg, as well as in the local museums of Kertch and elsewhere, other illustrations of the peculiar physical characteristics of the ancient macrocephali of the Euxine will be accumulated in confirmation of the early notices of the father of medicine. Meanwhile, however, all hopes of ascertaining any more precise details than the slight notice furnished by Captain Jesse of the Macrocephalic cranium seen by him in the Kertch Museum had been dissipated by the dispersion and wanton destruction of its treasures; and I had ceased to think specially of Crimean crania, when I was gratified by receiving the gift of a skull, including the lower jaw, brought from Kertch by a young Canadian physician, who had served on the medical staff of the British army during the Russian campaign. The skull, which, along with much delicacy, is characterised by some very noticeable peculiarities in its conformation, was described by the donor as that of a Circassian lady. In form it presented no correspondence with the Macrocephalic type to which my inquiries had been previously directed. The forehead is markedly vertical, and, in its general proportions, it is strikingly characterised as a brachycephalic cranium of unusual width at the parietal protuberances, while marked by great delicacy and beauty, especially in the facial bones.

A special interest attaches to the evidences of physical form, as well as of philological characteristics, pertaining to the tribes of the Caucasian area, owing to the factitious importance that has been assigned to certain of them in modern ethnology. It may not, therefore, be altogether valueless to put on record the facts connected with the recovery of the Crimean cranium in question, and to note the peculiarities of its form and measurements; though, from the mixed character of the population of Kertch, it would not be safe to assign the crania of its modern cemetery to any absolute ethnological group, or to make them the basis whereon to found data for classification, or for any comprehensive generalisation.

Dr Latham, in his "Varieties of Man," classes the nations and tribes of the area within the range of Mount Caucasus under the generic designation of Dioscurian Mongolidæ, including in its chief divisions the Georgians, the Lesgians, the Mizjegi, the Irôn, and the Circassians. He derives the term

Dioscurian from the ancient seaport of Dioscurias, where the chief commerce between the Greeks and Romans and the natives of the Caucasian range took place. According to Pliny, it was carried on by 130 interpreters—so numerous were the languages; and one striking characteristic of the locality, still noticeable, is the great multiplicity of mutually unintelligible tongues. This, therefore, is the idea designed to be conveyed by the term Dioscurian. Caucasian would have been a preferable, because more familiar and precise term, but it has been already appropriated as an ethnological division, in a way sufficiently confusing and indefinite, without adding thereto by the creation of such a contradictory union of terms as would arise from such a designation as Caucasian Mongolidæ—almost equivalent, in popular acceptation, to European Asiatics.

The use of both epithets, Caucasian and Mongolian, is traceable to Blumenbach; and the history of his adoption of the former supplies a curious example of a term subsequently employed as one of the most comprehensive heads of classification having its origin from the fewest possible premises. Among the captives taken by the Russians in one of their frequent inroads on the country lying between Mount Caucasus and the Euxine, was a Georgian woman, who was carried prisoner to Moscow, and died suddenly there. The body was made the subject of anatomical examination by Professor Hiltenbrandt; and the skull having been prepared, was subsequently presented to Dr Ash of St Petersburg. From him it passed into the hands of Blumenbach; and its peculiar symmetry and beauty appear to have made a lively impression on his mind. That this was not without good reason, appears from the following description of the Georgian cranium by Dr Lawrence:—

"The form of this head is of such distinguished elegance, that it attracts the attention of all who visit the collection in which it is contained. The vertical and frontal regions form a large and smooth convexity, which is a little flattened at the temples. The forehead is high and broad, and carried forward perpendicularly over the face. The cheek-bones are small, descending from the outer side of the orbit, and gently turned back. The superciliary ridges run together at the root of the nose, and are smoothly continued into the bridge of that organ, which forms an elegant and finely-turned arch. The alveolar processes are softly rounded, and the chin is full and prominent. In the whole structure there is nothing rough or harsh—nothing disagreeably projecting. Hence it occupies a middle place between the two opposite extremes, of the Mongolian variety, in which the face is flattened and expanded laterally; and the Ethiopian, in which the forehead is contracted, and the jaws also are narrow and elongated anteriorly."

Little could the poor Georgian captive dream of the posthumous honours and admiration that were to atone to her for her living wrongs. She has avenged herself on her European captors by introducing uncertainty and confusion into the science for illustrating which Blumenbach regarded her symmetrical cranium as a peculiarly valuable prize. It was in the Third Decade of his anatomical descriptions of skulls, published in 1795, that the skull of the fair Georgian was introduced, accompanied by a glowing description of its elegance and unequalled grace; and a reference to the beauty of the Georgian women, which, as his example proved, lives even in their fleshless bones. A comparison of the skull, with a cast of one of the most beautiful classic busts in the Townley Collection, seemed to the enthusiastic craniologist as though he had acquired the actual skull of the head from which the ancient marble was copied; and when placed alongside of the only Greek skull in his collection, the Georgian was superior to it, the Greek being next in rank.

Hence it was that Blumenbach adopted his Georgian skull as a typical cranium for the most perfectly developed division of the human species. In the same decade in which the Georgian skull appears, the term *Caucasian* is introduced in connection with it; and along with this term of classification appear also those of Mongolian and Ethiopian; and these, with the epithets Malay and American, subsequently added, formed the names of a quinary division of the human species, which he conceived his physical researches to have established. By the term Caucasian, Blumenbach meant no more than the adoption of a convenient name for his highest division of the human species, the typical characteristics of which were most completely epitomised in his symmetrical cranium. But the associations and historical traditions connected with Mount Caucasus supplied a tempting basis for theory and speculation. The mountain range was assumed by some as the central point for the origin of mankind; and the epithet derived from it is now associated with so many extravagant ideas, and so much loose and confused classification, that the vague uncertainty it has acquired is abundantly sufficient to justify its abandonment. When, however, Dr Latham substitutes the term Dioscurian for Caucasian, in its limited sense, as applicable to the inhabitants of the actual area of Mount Caucasus, he does so not only from different data to those employed by Blumenbach, but even in defiance of such analogies as their ascertained physical conformation seems to suggest. He accordingly admits that he occupies exceedingly debateable ground. "So long has the term Caucasian been considered to denote a type of physical conformation closely akin to that of the Iapetidæ, *i.e.* pre-eminently European, that to place the Georgians and Circassians in the midst of the Mongolidæ is a paradox. Again, the popular notions founded upon the physical beauty of the tribes under notice, are against such a juxtaposition; the typical Mongolians, in this respect, have never been mentioned by either poet or painter in the language of praise." Perhaps, however, the facts which justify Dr Latham in saying of Blumenbach's solitary Georgian skull, "Never has a single head done more mischief to science than was done in the way of posthumous mischief, by the head of this well-shaped female from Georgia," may have had their influence in tempting to the Caucasian paradox of his Dioscurian Mongols. The classification, at any rate, entirely ignores physical conformation, and rests on vocabulary analogies, confirmed by an opinion expressed by Mr Norris, of the Asiatic Society, that on the surer evidence of grammatical comparison, the closest philological affinity of the Dioscurian languages is with the Aptotic ones, of which the Chinese is generally accepted as the type.

It is scarcely necessary to say, that languages may belong to a different class from the people who speak them. Europe supplies abundant and well authenticated illustrations of this. An Englishman speaking Chinese does not thereby become a Mongol, nor will the adoption of the English tongue by the Chinese emigrants to Australia and elsewhere affect their essentially Mongolian physical characteristics. Dr Latham accordingly refers to the want of sufficient evidence for discussing the physical elements of classification in his Dioscurian Mongols. "Physiological objections," he observes, "based upon the symmetry of shape and delicacy of complexion on the part of the Georgians and Circassians, I am at present unable to meet. I can only indicate our want of osteological data, and remind my readers of the peculiar climatological conditions of the Caucasian range, which is at once temperate, mountainous, wooded, and in the neighbourhood of the sea; in other words, the reverse of all Mongol areas hitherto enumerated. Perhaps, too," he adds, "I may limit the extent of such objections as a matter of fact. It is only amongst the chiefs where the personal beauty of the male portion of the population is at all remarkable. The tillers of the soil are, comparatively speaking, coarse and unshapely."

The latter remark, whatever be its value, may be made of the tillers of the soil everywhere; but if the Georgian and Circassian mothers are generally as graceful and beautiful in form as the concurrent opinion of travellers affirms them to be, the perpetuation of anything approximating to a Mongol physical type in their sons, would be one of the greatest marvels in physiological ethnology. In the absence, however, of osteological data, the smallest contribution towards the accumulation of the requisite facts may have its value.

The history of the cranium to which I now direct attention is as follows:—Dr Michael Turner was present in the Crimea, and in active service on the medical staff during the Anglo-French invasion of 1855, and witnessed the capture of Kertch. At that period its population was estimated at between 7000 and 8000, and was composed of Tartars, Cossacks, Greeks, Russians, and a sprinkling from the tribes bordering on the shores of the Black Sea. More than two-thirds of the whole population of the Crimea are a mixture of the pure Asiatic

Mongol Tartar with the modified European Turk; and except among the nobles or murses, and partially among the population of the northern valleys, they abundantly indicate their Tartar origin in their features.

The antipathies which the mutual wrongs of Russian and Turk have created, have obliterated in the minds of the latter any idea of kindred with the Tartar or semi-Turkish population of the Crimea; and after the sack and pillage of the town of Kertch, the Turkish troops carried their violence so far as to open and spoil the graves in the Christian cemeteries; and on finding trinkets and relics in some of the first they opened, a general desecration ensued. The articles found consisted of rings, beads, and amulets, and also of crucifixes, and images of the saints; and these were sought for, and appropriated by the Turkish soldiers, with the utmost indifference to the condition in which they left the ravished occupants of the desecrated graves. While strolling in the neighbourhood of the city where such shameful spoliation had been carried on, Dr Turner passed through a large cemetery, which he was led to believe had been confined exclusively to members of the Greek Church, from the number of large marble crosses heading the graves. Most of the latter were opened, and rifled of such of their contents as could tempt the cupidity of the spoilers; and the skeletons and partially desiccated remains of their former occupants lay strewed about the ground. On looking into one of the open graves which had been thus despoiled, he was tempted to examine the nature of the sepulture, as the body still remained in its original position; and also to ascertain whether the marauders had left anything of value behind. He accordingly jumped into the grave, and turning over the loose soil with his hands, he was struck, on uncovering the head, by its long black hair and beautiful teeth. The body was not yet returned to the dust, so that the interment was one of no very remote date from that of the disturbance of what cannot properly, under such circumstances, be called its last resting-place. The muscles, which still remained on the forehead, were dry and contracted; and across the forehead, and round the head, was a broad gold fillet, sufficiently indicating that the grave was tenanted by one who had occupied a high social rank. No other ornaments or relics were observed, the whole of these having doubtless been removed by the original riflers of the grave. Dr Turner did not consider it a very serious aggravation of the desecration to which the dead had already been subjected, to possess himself of the skull, which struck him as one peculiarly marked with indications of former delicacy and beauty; and through the kind intervention of my friend Dr C. W. Covernton, it has since been transferred to me.

From a comparison with other skulls procured by him, Dr Turner at first inclined to the opinion that he had acquired the cranium of a Greek lady. The breadth at the parietal protuberances, however, along with other marked features, differ essentially from the Greek type of head; and as there were many Circassians among the wives of the most influential and affluent families in the city, the probabilities, he conceives, are, *a priori*, in favour of its being ascribed to a people celebrated for the beauty of its females, and for their frequent introduction both to Turkish and Græco-Russian households around the Euxine. An elaborately sculptured, but broken marble cross, at the head of the grave, added additional proof that the once loved and lost beauty of some Kertch household, whose remains were subjected to such indignities, had been ranked during her lifetime among the finest porcelain of human clay. Under the peculiar system which prevails in oriental households, however, and by which Christian as well as Ottoman alliances are influenced, a wide area is embraced within the possible origin of the beauties who adorn such Eastern homes; and a comparison of the most strikingly marked characteristics of this head with the varying types of cranium pertaining to what may be regarded, even in some respects philologically, as the European ethnic area, would rather suggest its classification among Armenian than Circassian forms. The materials, however, for arriving at any very definite conclusion are limited, and perhaps inadequate for positive generalisations; and it may suffice to put on record such minute descriptions and measurements as may afford the means of future comparison.

The skull, as already indicated, is that of a female, of fully thirty years of age. The bones of the face are characterised by great delicacy. The zygomata are slight, and enclose a space proportionally small by the zygomatic arch. The face is altogether small for the head, giving the idea of a considerable breadth of forehead, though it will be seen that the parietal diameter is in greater excess than usual when compared with the frontal diameter. The teeth, the beauty and completeness of which attracted the attention of Dr Turner when first exposed in the cemetery at Kertch, have since mostly fallen out; but with the exception of one decayed molar, such as remain fully accord with his description, and with the delicacy of the superior and inferior maxillaries. The forehead is smooth, with no projection of the frontal sinuses, and no depression above the nasal suture, but with marked frontal protuberances at the upper angles of the forehead. The occipital protuberance is slight, and the profile of the calvaria exhibits a markedly vertical aspect, both in its frontal and occipital outlines. The frontal bone passes somewhat abruptly from the forehead to the top of the skull, thereby giving a square form to the profile, instead of the more usual arched curvature; so that, with the nearly vertical occiput, the cranium has a singularly compact outline when viewed in profile. The parietal bones are large, with a gradually increasing protuberance to their greatest diameter, a little behind the line of the mastoid processes. Owing to this, the outline of the vertical aspect presents somewhat the form of a truncated wedge, narrowing gradually, and with a nearly uniform diminution, until abruptly rounded off into the forehead at the frontal protuberances.

The following are the most characteristic measurements of this skull:—

Longitudinal diameter, . .	6·7
Parietal diameter, . . .	5·7
Frontal diameter, . . .	3·8
Zygomatic diameter, . . .	4·4
Vertical diameter, . . .	4·7
Intermastoid arch, . . .	14·3
Intermastoid line, . . .	3·7
Length of face, ...	6·2
Horizontal circumference, . .	19·7

Dr J. Aitken Meigs has remarked, in his "Cranial Characteristics of the Races of Men," chiefly founded on data supplied by the Morton Collection in the Academy of Sciences at Philadelphia: "The extreme south-eastern section of the European ethnic area, occupying mainly the table-land of Iran, is represented in the Morton Collection by six Armenian, two Persian, and one Affghan skull. A general family resemblance pervades all these crania. They are all, with one exception, remarkable for the smallness of the face, and shortness of head. In the Armenian skull, the forehead is narrow and well formed, the convexity extending upwards and backwards towards the parietal protuberances, and laterally towards the temporal bones. The greatest transverse diameter is between the parietal bones. This feature, combined with the flatness of the occiput, gives to the coronal region an outline resembling a triangle with all three angles truncated, and the base of the triangle looking posteriorly. In fact, the whole form of the calvaria is such as to impress the mind of the observer with a sense of squareness and angularity. The dimensions of the orbits are moderate; the malar bones small, flat, and retreating; zygomatic processes slender; and the general expression of the face resembling that of the Circassians, from which latter it differs in being shorter." On nearly all those points, the Kertch skull closely corresponds to this description of Armenian cranial characteristics. The only noticeable exceptions are in the orbits, which may be described as somewhat large, but with their perpendicular diameter the greatest; and in the length of the face, which has more of the assigned Circassian dimensions. The formation of the lower jaw indicates a delicately pointed and small chin. Viewed altogether, the peculiar features of this skull are well defined, and sufficiently characteristic to enable an experienced craniologist to assign it, with little hesitation, to the Iranian group, with its included Georgians, Lesgians, Circassians, and Armenians. Of those the last named—to which the Kertch cranium seems by its most prominent peculiarities to belong—possesses some characteristics of peculiar interest. In his "Varieties of Man," Dr Latham places the Armenians foremost among his "unplaced stocks;" but regarding them from a philological point of view, he seems to consider them as in some respects presenting indications of a link between the Indo-European and the Semitic groups. But he also adds: "It is through the Armenian that the transition from the Mongolidæ to the Atlantidæ is most likely to be recognised." Obtained as the skull now described has been, under peculiar and somewhat unique circumstances, and with a minuteness of evidence relative to the social condition and the vital characteristics originally pertaining to her whose sepulture was involved in the ravages of the Crimean war, which led to its acquisition, the facts recorded in this paper may possess some slight value as a contribution to data now accumulating from the labours of many independent workers, and destined ultimately to establish physical ethnology on a sure and well-determined basis.

REVIEWS AND NOTICES OF BOOKS.

Archaia; or, Studies of the Cosmogony and Natural History of the Hebrew Scriptures. By J. W. Dawson, LL.D., F.G.S., Principal of M'Gill College, Montreal. 1860. 8vo., pp. 400.

At the present time, when there is a tendency among men of science to ignore the testimony of Scripture—when, in a volume of Essays and Reviews which has been recently published, there is an attempt to annihilate the authority of the Bible as the inspired Word of God, to reject all miracles as incapable of proof and repugnant to reason, and even to undermine our faith in God as the Creator—it is pleasing to find a man like Principal Dawson, of high geological reputation, coming forth to vindicate the inspiration of the Bible, and to show the accordance between its statements and the facts of modern science. It is not for us to enter upon the question of Bible theopneustia; that has been ably proved by others. Our desire is to show that there is no discrepancy between science and religion. The Word of God and the works of God are in perfect harmony. God is the author of both; and, when we regard their common origin, we may rest assured that they will never be in opposition. We have no fear of true science. It has always done its duty well. It is science falsely so called that attempts to oppose the truth of the Bible. Such oppositions are the result, not of increase, but of defect of knowledge. They are advanced by men who profess themselves to be wise, but who are in reality fools. Dr Dawson, as a Christian man, takes the Scripture as his guide, and he has brought his biblical knowledge to bear on the facts of geology. The present work is the result of a series of exegetical studies of the first chapter of Genesis, in connection with the numerous incidental references to nature and creation in the other parts of the Holy Scriptures. He says that he cannot "hope for the approval of that shallow school which decries Bible philosophy as a thing of bygone times, and attempts to raise an insurmountable barrier between the domains of faith and reason, by excluding from nature the idea of creative power, or from religion the noble cosmogony of the Bible. His utmost hopes will be realised, if he can secure the approbation of those higher minds in which the love of God is united with the study of His works."

In his introductory remarks the author alludes to the early history of the Bible—of the Revelation made to Moses and the prophets, and to the writers in the time of our Saviour. He then goes on to say,—"Nor is the modern history of the Bible less wonderful; exhumed from the rubbish of the middle ages, it has entered on a new career of victory; it has stimulated the mind of modern Europe to all its highest efforts, and it has been the charter of its civil and religious liberties. Its wondrous revelation of all that man most desires to know, in his past, in his present, and in his future destinies, has gone home to the hearts of men in all ranks of society, and in all countries. . . . Explain it as you may, the Bible is a great literary miracle, and no amount of inspiration or authority that can be claimed for it is more strange or incredible than the actual history of the book."

The author considers first the object, character, and authority of the scriptural view of the cosmos, and he concludes—1. That the Mosaic cosmogony must be considered, with the prophecies of the Bible, as claiming the rank of inspired teaching, and must depend for its authority on the maintenance of that claim. 2. That the incidental references to nature in other parts of Scripture indicate at least, the influence of those earlier teachings, and of a pure monotheistic faith, in creating a high and just appreciation of nature among the Hebrew people.

The general views of nature contained in the Holy Scriptures are next considered. It is shown that the Scriptures assert invariable natural law and constantly recurring cycles in nature; that they recognise progress and development, purpose, use and special adaptation in nature, as well as the law of type or pattern. After these preliminary matters, the author proceeds to consider the statements *seriatim* in the first chapter of Genesis. He remarks, "No studied introduction precedes the sacred narrative. No attempt is made to prove the existence of God, or to disprove the eternal existence of matter. The history opens at once with the assertion of the great fundamental truth, which must ever form the basis of true and sound philosophy—the production from non-existence of the material universe by the eternal self-existent God." He paraphrases the first and second verses thus:—"At a far-distant time, Elohim, the triune God, created the material of the heavens and the earth. After its creation, the earth was still without organised inhabitants. It was covered with a dense and heterogeneous mantle of vapours, and it was entirely destitute of solar light and heat, but processes preparatory to its being perfected and inhabited were in progress."

After alluding to a self-luminous atmosphere round the earth as being the first source of light, he takes up the question as to days or hours of creation, and says, "In reviewing all the train of reasoning into which the term Day has led us, it appears that, from internal evidence alone, it can be rendered probable that the day of creation is neither the natural nor the civil day. It also appears that the objections urged against the doctrine of day-periods are of no weight when properly scrutinised, and that it harmonises with the progressive nature of the work, the evidence of geology, and the cosmological notions of ancient nations." The Atmosphere and the Dry Land next call for discussion, and the First Vegetation is brought under notice with the remark that "plants were created each kind by itself, and that creation was not a sort of slump work, to be perfected by the operation of a law of development, as fancied by some modern speculators. In the assertion of the distinctness of species and the production of each by a distinct creative art, Revelation tallies perfectly with the conclusions of natural science, which lead us to believe that each species is permanently reproductive, variable within narrow limits, incapable of permanent intermixture with other species, and a direct product of creative power. . . . On the theory of long creative periods, the statement as to the third day must refer to the first introduction of vegetation in forms that have long since ceased to exist. Geology informs us that, in the period of which it is cognisant, the vegetation of the earth has been several times renewed, and that no plants of the older and middle geological periods now exist. We may therefore rest assured that the vegetable species, and probably also many of the generic and family forms of the vegetation of the third day, have long since perished and been replaced by others suited to the changed condition of the earth."

The correspondence between the geological and Mosaic records is well seen in the character of the animals found in the fossiliferous rocks. The palæozoic, mezozoic, and tertiary epochs are considered as being markedly referred to in the Scripture account of the succession of animals. Man is the climax of the creation. "The Bible knows but one species of man. It is not said that man was created after their species, as we read of the groups of animals. Man was made male and female, and but one primitive pair is introduced to our notice. . . . Nor does the Bible allow us to assign a very high antiquity to the origin of man. Its careful genealogical tables admit of but very narrow limits of difference of opinion as to the ages of the human world or æon; and especially of the deluge, from which man took his second point of departure." The last provision in time, according to Forbes, was completed by the coming of man; he stands unique in space and time. No proof exists of the production of a new species since the creation of man; and geological evidence points to him and a few of the higher mammals as the newest of the creatures. Thus far science leads us, but the after change in man and his fallen condition are only learned by Revelation. No doubt there is sufficient evidence to show the most sceptical naturalist the disjointed state of the present relations of man to nature, and the violations of symmetry occur everywhere. But while science leads us to suspect the fallen condition of man, it leaves us henceforth to the teaching of Revelation. Dr Dawson seems to adopt the view of Hugh Miller as to the seventh day being the modern or human era of geology, the Redemption rest of the Creator. The subject of the unity of man is taken up, and the views of Agassiz are combatted. Scripture and science are shown to be in harmony both as to the unity and antiquity of man. In the concluding chapter a comparison is made between the teachings of Geology and Scripture, under the following heads:— 1. Scripture and science both testify to the same fact, that there was a beginning. 2. Both records exhibit the progressive character of creation. 3. Both agree in affirming that since the beginning there has been but one great system of nature. 4. The periods into which geology divides the history of the earth are different from those of Scripture, yet, when properly understood, there is a marked correspondence. 5. In both records the ocean gives birth to the first dry land, and it is the sea that is first inhabited, yet both lead at least to the suspicion that a state of igneous fluidity preceded the primitive universal ocean. 6. Both concur in maintaining what is usually termed the doctrine of existing causes in geology. 7. Both agree in assuring us that death prevailed in the world ever since animals were introduced. 8. In the department of final causes, as they have been termed, Scripture and geology unite in affording large and interesting views. 9. Both records represent man as the last of God's works, and the culminating point of the whole creation.

For the arguments on which these points of accordance are founded, we must refer to the work itself. Although there may be some difficulty in agreeing with our author in all his interpretations and views, still we consider his volume as one well worthy of careful perusal and study, and as admirably calculated to counteract the sceptical theories of the present day, and to lead to a patient study of science in the light of Revelation. The work is written by an able geologist and a good Biblical scholar—by one who loves science, and who loves the truth—by one who contemplates nature with the eye and heart of a Christian philosopher, and who is admirably qualified to superintend the scientific studies of the youth in the college over which he presides.

The Botanist's Guide to the Counties of Aberdeen, Banff, and Kincardine. By G. DICKIE, A.M., M.D., Professor of Botany in the University of Aberdeen. 1860. 12mo, pp. 344.

Local Floras are of great importance as regards geographical botany, and they are also valuable aids to the student in the prosecution of his practical researches. The present work is one of great interest, inasmuch as it embraces one of the most alpine districts of Britain, containing many rare species, and constantly visited by naturalists. The plants of Aberdeenshire and the neighbouring counties have occupied the attention of observers for at least 100 years. Dr David Skene, a contemporary and correspondent of Linnæus, studied the zoology and botany of the district. Since his time, the botany of Aberdeenshire has occupied the attention of many zealous naturalists, among whom may be noticed Professors Beattie and Knight, Dr Alexander Murray, Professors Graham and Balfour, Mr Hewett C. Watson, Mr Gardiner, and Professor Macgillivray. In 1856, Dr Dickie published a Flora of Aberdeen, embracing a range of about twelve miles south-west and north. The "Natural History of Deeside," by Macgillivray, was published after his death, and was privately circulated, through the liberality of His Royal Highness the Prince Consort.

The district included in the present work has been the scene of many herborizing excursions, and the mountains of Ballater and Braemar have been trode in sunshine and in mist by numerous botanical collectors, who have, by their labours, added to the Flora of Scotland, and enriched the British Herbaria with many rare specimens. These wild localities used to be freely traversed by all, but within the last twenty years the increase of deer-stalking has caused many restrictions to be imposed on travellers, and has interrupted in no small degree the researches of naturalists, who have often been brought into unpleasant collision with gillies and gamekeepers, and even with parties whose education and position ought to have led them to sympathise with the votaries of science.

Dr Dickie's work is the result of many years of patient investigation, and it is executed with his usual clearness and accuracy. It embraces both the Phanerogamous and the Cryptogamous plants of the district, and it contains interesting details as to the physical characters of the counties, and a full account of the range of species. Valuable meteorological tables are given, and a plan of the distribution of species in the Agrarian, Super-agrarian, Infer-arctic, Mid-arctic, and Super-arctic Zones.

The following table, compiled from his own observations, affords an idea of the total number of species, and the prevailing types at different altitudes. The letter indicates the respective types: B. British; S. Scottish; H. Highland.

Bennachie, 1700 feet,	10 B..	. . .	Dicotyledons.
	4 B.,	. . .	Monocotyledons.
Lonach, 1836 feet,	9 B., 2 S., 2 H.,	total,	13 Dicotyledons.
	8 B.,	. . ,,	8 Monocotyledons.
Khoil, 2000 feet,	8 B., 2 S., 4 H.,	,,	14 Dicotyledons.
	4 B., 1 H.,	. ,,	5 Monocotyledons.
Buck of Cabrach, 2264 feet,	5 B., 1 S., 2 H.,	,,	8 Dicotyledons.
	3 B., 1 H.,	. ,,	4 Monocotyledons.
Mount Battock, 2563 feet,	3 B., 2 H , 1 S.,	,,	6 Dicotyledons.
	1 B., 1 H,,	. ,,	2 Monocotyledons.
Mount Keen, 3125 feet,	2 B., 2 H.,	. ,,	4 Dicotyledons.
	1 B., 3 H.,	. ,,	4 Monocotyledons.
Lochnagar, 3800 feet,	1 S., 3 H.,	. ,,	4 Dicotyledons.
	1 B., 3 H.,	. ,,	4 Monocotyledons.
Ben-a-Buird, 3900 feet,	2 B., 4 H.,	. ,,	6 Dicotyledons.
	1 B., 5 H.,	. ,,	6 Monocotyledons.
Ben Muic Dhui, 4300 feet.	3 H.,	. . ,,	3 Dicotyledons.
	4 H.,	. . ,,	4 Monocotyledons.

"The instances selected are the very summits of the respective mountains; places where a complete list of species can be readily got. It will be obvious at a glance, that there is a rapid decrease in the number of species belonging to the British type, the last to disappear being *Calluna vulgaris;* of the Scottish type, *Empetrum nigrum* ascends highest, viz., to 4100 feet. On the other hand, the Highland type increases steadily in numbers till at last, species belonging to it constitute alone the scanty flora. The mid-arctic zone is that in which there is the greatest development of Highland species. The proportion of Monocotyledons to Dicotyledons at different altitudes is also worthy of notice. In the entire Flora of the county of Aberdeen, the proportion is 1 to 2·4;

at 3125, 3800, and 3900 feet, they are equal; and at the highest point they are 1·3 to 1. The Monocotyledons, therefore, increase in proportional numbers as we ascend."

The flowering plants are arranged in 74 natural orders, according to the system of Decandolle. The English names are added, also the time of flowering, the type to which they belong, and their range in Britain, both latitudinally and altitudinally. A list of introduced species is appended. Catalogues of ferns, mosses, lichens, fungi, and algæ are added, with the localities of the species. In the nomenclature of the mosses, Wilson's "Bryologia Britannica" is followed; in the case of the lichens, Schærer's "Enumeratio Lichenum."

To show the interesting information conveyed in this excellent little work, let us take a single example, the common Scotch fir:—

"*Pinus sylvestris*, Linn. (Scotch Fir.)

"Perennial—Flowers in May and June—Scottish type. Range in Britain, 56° to 59°; 1500 to 2200 feet.

"Truly wild examples of this tree are at present only found in the interior of the district; the numerous remains found in peat-bogs in many parts where it does not now grow, indicate a more general distribution of it in a former epoch. This remark applies not merely to the lower parts, but also to the higher and more inland localities. The stems of the plant are to be seen in peat-mosses at high altitudes, where such trees cannot grow at the present day. Mr Watson (*Cybele Britannica*, vol. ii. p. 410) alludes to a trunk with a girth of 8 feet, at 550 yards of elevation in Aberdeenshire, the upper limit of *fir woods* being at present about 1950 feet, where the trees attain far less size. In 1842 there existed in Mar Forest an example of this tree, measuring in girth at the base 22 feet 4 inches; the age unknown. In the same year I found that some stumps of this species in the forest of Balachbuie, having 120 annual zones, measured 8 feet in circumference at the base; taking such as a standard, the patriarch of the Mar Forest must have been more than 300 years old. The results of numerous observations lead to the conclusion that the rate of growth of this valuable tree continues steadily up to seventy years, diminishing from that period to ninety and upwards. In very aged trees, the annual zones near the outside of the trunk are very thin, and not easily counted. A remarkable distortion of the trunks of the Scotch fir occurs in some localities; the stems present the most fantastic shapes, and the letter S represents a form frequently assumed. Such effect is usually supposed to be produced by the drifting of snow upon the trees when young, and while their stems are still slender and flexible. In the upper parts of Glen Quoich, the numerous dead and bleached stems of the Scotch fir have a very remarkable spiral twist in the wood;

it is probable this is owing merely to the continued action of eddy winds upon the trees during their whole life. Such spiral arrangement of the wood is not a natural structure. It has been observed also in the forests of north-eastern Europe, and I believe the same explanation given."

Dr Dickie's Flora is a model of what a local list ought to be. The statements are condensed, practical, and useful. Nothing is given but what bears directly on the distribution of the species in the district; and the accurate observations as to their range add very much to the value of the work. We recommend it particularly to all those who visit the alpine districts of Scotland in search of their floral treasures.

The Life of William Scoresby, M.A., D.D., F.R.SS. L. & E., Corresponding Member of the Institute of France. By his Nephew, R. SCORESBY-JACKSON, M.D., F.R.S.E. 8vo, pp. 406. London, 1861: T. Nelson and Sons.

The object of this book, as stated in the preface, is to give a concise history of a man of a singularly active and observant mind, who was ever careful to record and preserve his observations; who in early life enlarged the sphere of his researches by repeated voyages; was the first to make an accurate survey of the east coast of Greenland, and who penetrated further north than any of his contemporaries; who devoted the latter half of his life to the moral instruction and amelioration of his fellow-creatures; a philosopher, whose acute intellect embraced some of the most subtle subjects of physical science; a sincere believer and candid advocate of religious truth; and withal a zealous and indefatigable practical philanthropist. The work is replete with interest and instruction, and is compiled in part from an autobiography left by Dr Scoresby.

Dr Scoresby was born on 5th October 1789, and was early initiated in the adventures of a seafaring life by his father, who commanded one of the Whitby ships engaged in the Greenland whale fishing. In his early life, as well as in later years, he prosecuted his studies at the University of Edinburgh, where he became acquainted with Professor Jameson and other scientific friends. He then embraced the calling which his father had long followed, and he made repeated voyages to the Arctic regions. These voyages, although intended for commercial purposes, were also made subservient to science, and led to the publication of a valuable work on the Arctic Regions, which established his scientific reputation, and introduced him to men of the highest eminence, and various scientific societies both at home and abroad.

He was an enterprising and bold navigator, and did much to advance our knowledge of Arctic phenomena. His magnetic investigations, and his observations on the waves and currents of the Atlantic, were particularly valuable, and placed him in a high position as a man of science. His final voyage to the northern regions took place in 1823. Subsequently to this he abandoned a sea life, became a candidate for orders in the English Church, and he continued to perform the duties of a clergyman in various parts of England, until his death at Torquay on 21st March 1857. During his ministerial life he made trips to America and Australia for the sake of his health, as well as for the purposes of science.

The details of his adventurous and chequered life are given in an interesting manner by his nephew, and they amply reward a diligent perusal. The zeal and earnestness of Dr Scoresby in his Arctic explorations, his scientific research, the discoveries he made and the striking incidents of his sea voyages, are well portrayed; and his Christian labours as an English clergyman are faithfully recorded. We consider the work as a valuable addition to the department of biography.

Elements of Agricultural Chemistry. By THOMAS ANDERSON, M.D., Professor of Chemistry in the University of Glasgow, and Chemist to the Highland and Agricultural Society of Scotland. 12mo, pp. 299. Edin.: A. & C. Black, 1860.

The object of the present work, as stated in the preface, is to offer to the farmer a concise outline of the general principles of agricultural chemistry. Its aim is strictly elementary, and unnecessary technicalities have been avoided, so as to bring it within the grasp of the farmer. The author, from the position which he occupies, has acquired a thorough knowledge of the wants of the farmer; and he has accordingly dwelt specially on those departments of the subject which bear more immediately on the every-day practice of agriculture—such as the composition and properties of soils, the nature of manures, and the principles by which their application ought to be governed.

After giving a short review of the progress of agricultural chemistry, the author writes: "Notwithstanding all that has recently been done, it must not be forgotten that we have scarcely advanced beyond the threshold, and that it is only by numerous and frequently-repeated experiments that it is possible to arrive at satisfactory results. Agricultural inquiries are liable to peculiar fallacies due to the perturbing influence of climate, season, and many other causes, the individual effect of which can only be eliminated with difficulty; and much error has been introduced by hastily generalising from single experiments, in place of awaiting the results of repeated trials. Hence it is that the progress of scientific agriculture must necessarily be slow and gradual, and is not likely to be marked by any great or startling discoveries. Now that the relations of science to practice are better understood, the extravagant expectations at one time entertained have been abandoned, and, as a necessary consequence, the interest in agricultural chemistry has again increased; and the conviction daily gains ground, that no one who wishes to farm with success can afford to be without some knowledge of the scientific principles of his art."

The subjects treated of in the volume are the organic, proximate, and inorganic constituents of plants; the food of plants; the chemical and physical character of the soil, with its improvement; the principles of manuring, and the various kinds of manures; the rotation of crops; and the feeding of farm stock. The farmers in Scotland are proverbially men of high intelligence, and of great practical acumen. They know well the advantages of scientific education, and it is hoped that they will support the efforts which are now being made by the Highland and Agricultural Society to carry out a proper curriculum of study, and an efficient examination for agricultural students. It is important to be able to place in the hands of such students a condensed treatise like the present, drawn up by an able chemist, who is eminently qualified to give correct instruction in reference alike to scientific and practical agriculture. We highly recommend the work. It ought to be in the hands of every student of agriculture, as a safe guide to him in his researches.

Flora Adenensis; a Systematic Account, with Descriptions of the Flowering Plants hitherto found at Aden. By THOMAS ANDERSON, M.D., F.L.S., H.M. Bengal Medical Service. Published in the "Journal of the Proceedings of the Linnean Society." Longman and Co. London, 1860.

The author of this Flora was distinguished as a zealous student of botany at the University of Edinburgh, and he continued to prosecute the science when he entered the medical service in India. He has contributed valuable papers to the Indian journals; among the rest a Flora of Lucknow. Since his return to Britain, he has been engaged in describing the Indian Acanthaceæ at Kew, and in publishing the Flora of Aden. He has now returned to India to occupy the position of superintendent of the Botanic Garden at

Calcutta. "Much attention has of late years been directed to the military station of Aden, owing to its rapidly increasing importance, both in a political and commercial point of view; and now that it is visited weekly by the large steamers in their course to India, China, and Australia, its name has become as familiar as that of any of our Eastern settlements. From its commanding position at the entrance of the Red Sea, and from its forming an indispensable link in the chain of communication with our Eastern Empire, the importance of the settlement will increase with the development of our Indian possessions.

"Aden is a small rocky peninsula, in many features resembling our other stronghold, Gibraltar; and is situated on the southern coast of Arabia, in 12° 47′ N. Lat., and 45° 10′ E. Long. The maritime region called Tehama, of which it is a promontory, is a sandy barren tract from 20 to 100 miles in breadth, extending along the shore of the Red Sea, from a point a little east of Aden the Gulf of Akaba. A mountainous region, of 4000 to 7000 feet in elevation, rises immediately beyond; this, from its height, attracts a considerable portion of the moisture borne from the Indian Ocean by the north-west monsoon; and thus, enjoying a climate favourable to the growth of luxuriant cereals and fruits, it has for ages been called the Happy Arabia. It forms a striking contrast to the sterile Tehama, in the southern portion of which rain but rarely falls, while towards its northern extremity it is quite unknown. The few streams that enter from the mountains of Arabia Felix are speedily lost in its arid sands; cultivation is therefore confined to the vicinity of the few towns and villages, and is dependent on a precarious supply of water from wells. The area of the Aden peninsula is about fifteen miles, its greatest breadth being five miles, and its least three. It is connected with the Arabian coast by a narrow sandy isthmus, covered at high spring-tides; but formerly it was probably an island, since the whole district is of recent origin, being evidently a raised sea-beach; as is shown by the remains, twenty-three miles inland, of the ancient seaport of Mooza, formerly frequented by the Phœnicians. The peninsula is entirely composed of volcanic rocks of apparently great age, forming numerous precipitous peaks and narrow ridges, which on the southern and eastern sides rise from the sea in inaccessible cliffs, attaining their greatest elevation, 1775 feet, in the peak 'Jibeel Shumshum.' On the eastern side, and towards the isthmus, is a considerable depression, the crater of the volcano, surrounded on nearly all sides by high walls of rock and cinder. From the serrated ridge Jibeel Shumshum, numerous narrow valleys, shut in by precipices, radiate on all sides towards the sea, in which some end abruptly, while on the northern side others widen out into the limited sea-beach.

"The only patches of vegetation occur at the base of these gorges, just above the sea-line; and the loose and tolerably fertile soil accumulated there consists of scoriæ mixed with sand, and the detritus washed from the rocks above by the torrents which rush down every ravine after the rare but heavy falls of rain. Along the cliffs utter sterility reigns, except where a ledge of rock or a mass of cinder has allowed the accumulation of sufficient earth to afford sustenance to a few straggling bushes of *Capparis galeata* or *Adenium obesum*.

"In so low a latitude, the sun shines with intense force nearly throughout the year; and at Aden the solar power is increased by every peculiarity of physical conformation and climate. The undisturbed atmosphere stagnates in the walled-in valleys, where a deathlike stillness always reigns. The black and naked rocks absorb by day the scorching rays transmitted through an ever cloudless sky, only to radiate the pent-up heat by night, thus confining to the shore the cool but feeble breezes that occasionally spring up from the Indian Ocean. Accordingly, even in December, when the sun's power is at its lowest, Dr Hooker found the temperature of the soil at 107° Fahr. a few feet below the surface. In the hotter seasons of the year, the sun, even in the early morning, is overpowering, and above the rocks the air flickers from the intense heat, while all distant objects are distorted by an imperfect mirage. Almost perpetual drought is of necessity the concomitant of such a climate as I have described; and accordingly the annual rainfall at Aden never exceeds six or seven inches, this scanty amount being spread over the period between October and the end of April, while occasionally none falls for a year and a half. Still, Aden is not considered unhealthy, even to Europeans, who seem to become soon accustomed to the heat; and so great is our power of adaptation to circumstances, that after a residence of a year or two, the climate is spoken of as cool and pleasant from October to the end of March, and as bearable during the remainder of the year.

"The vegetation of Aden closely resembles that of Arabia Petræa, of which it is evidently the southern extension. It is eminently of a desert character, the species being few in number (only ninety-four), and being quite disproportioned to the number of genera and natural orders, even when the flora is compared with those of localities having similar areas and similar relations to the mainland. Most of the species are limited in the number of individuals, a few only of the more arid forms predominating. *Dipterygium glaucum*, six or seven species of *Capparidaceæ*, *Reseda amblyocarpa*, *Cassia pubescens* and *obovata*, *Acacia eburnea*, and a few *Euphorbiaceæ*, are the only common plants; and some of these are so plentiful that in many places they abound to the exclusion of all other plants. The other species are either very local, or sparingly scattered over the peninsula.

"All the species are more or less peculiar in their habits; and some are so strange in their appearance as to constitute the anomalies of the natural orders to which they belong. As examples may be enumerated—*Sphærocoma Hookeri* among *Caryophyllaceæ ; Adenium obesum*, with its almost globular fleshy trunk, naked branchlets bearing a tuft of leaves and an umbel of beautiful flowers; *Moringa aptera*, in which the leaves are reduced to long subrigid raches; the prickly *Jatropha spinosa*, and, strangest of all, the *Æluropus arabicus*, a grass with short spiny leaves, so sharp that it was with the greatest difficulty I could procure specimens of it. The bright green colour, which forms so pleasing a feature of the vegetation of the temperate and moist tropical regions of the globe, is quite unknown at Aden. Here foliage is reduced to a minimum, and the superfluous moisture given off by leaves in less arid climates is stored up in fleshy stems against seasons of long-continued drought. With the exception of some *Capparidaceæ* and *Reseda amblyocarpa*, all the plants have either glaucous whitened stems and leaves, or are completely covered with a hoary pubescence.

"Aridity, while reducing the amount of cellular tissue, has also favoured the production of spines; and though in many cases the development has not attained actual spinosity, still the rigid or distorted branches, and asperities of stem and leaf, bear witness to the modifying influence of the climate. Of the ninety-four species that constitute the flora, sixteen bear sharp thorns on some part of their structure. In some the leaves terminate in sharp, recurved hooks; in others the stipules are spinous; in a few the bracts are prickly; and in *Lycium Europæum* and *Euphorbia cuneata*, the short, stiff branches are terminated by short thorns. Several species yield gums or resinous matter, and their stems frequently become encrusted by these exudations, probably caused by the bark cracking from exposure to the great heat of the sun. I have observed resinous substances accumulated in various quantities on *Balsamodendron opobalsamum, Acacia Edgeworthii, Adenium obesum*, and the shrubby *Euphorbiæ*. All the *Capparidaceæ* (with the exception of *Mœrua Thomsoni*), *Dipterygium glaucum, Reseda amblyocarpa*, the *Compositæ*, and a few others, are characterised by more or less pungency or aromatic odour, qualities always possessed by plants of desert regions.

"Of the ninety-four species composing the florula, fourteen, or a little less than a sixth, are endemic, and one constitutes a new genus confined to Aden. They are as follows:—

Cleome paradoxa, R. Br.
——— *pruinosa*, T. Anders.
Mœrua Thomsoni, T. Anders.
Sphærocoma Hookeri, T. Anders.
Hibiscus Welshii, T. Anders.
Sterculia arabica, T. Anders.
Tavernieria glauca, Edgew.
Acacia Edgeworthii, T. Anders.
Ptychotis Arabica, T. Anders.
Convolvulus sericophyllus, T. Anders.
Anarrhinum pedicellatum, T. Anders.
Campylanthus junceus, Edgew.
Lavandula setifera, T. Anders.
Euphorbia systyla, Edgew.

The remaining eighty species have an extensive geographical distribution, fourteen occurring over all the barren part of the globe."

The work is executed with great ability. The character of the genera and species are well given, and the synonymes are worked out most carefully. It reflects the highest credit on Dr Anderson, and stamps him as one of the rising botanists of the age.

Flora of Cambridgeshire; or a Catalogue of Plants found in the County of Cambridge, with references to former Catalogues, and the Localities of the rarer Species. By CHARLES CARDALE BABINGTON, M.A., F.R.S., F.L.S. John Van Voorst, London 1860. 12mo, pp. 327.

No one has done more than Mr Babington to advance the study of British plants. His "Manual of British Botany" has long been a standard work, and is the most compendious field-book for the botanical student. The present volume contains a list of the plants of Cambridgeshire, with a description of the topographical peculiarities of the county, and a tabular view of the geographical distribution of species. The list contains about 950 flowering plants and ferns. A map is added, showing the different botanical districts into which the county may be conveniently divided. Ray published a list of Cambridgeshire plants about 200 years ago, and his work was succeeded by "Relhan's Flora Cantabrigiensis," the last edition of which appeared about 40 years ago. There was great need, therefore, of a new list brought up to the state of science at the present day. This has been most ably accomplished by Mr Babington. His valuable local Flora ought to be in the hands of every one who desires to know the plants of Cambridgeshire.

"The county of Cambridge is about 50 miles in length from north to south, and its greatest breadth is 25 miles. It is said to contain nearly 550,000 acres. It lies wholly between the 52d and and 53d parallels of latitude, and the town of Cambridge is situate five miles to the east of the meridian of Greenwich. We learn from the observations of the Rev. L. Jenyns, that the mean temperature of the seasons is—Spring, 47°·18 Fahr.; Summer, 60°·87; Autumn, 49°·86; and Winter, 38°·09. The mean annual range of the barometer is 1·890 inches. The mean fall of rain is about 22·5 inches. Deep drifting snow is not common, so that the roads are seldom obstructed by it. The prevalent winds are from the south-west and north-west; but cold north-easterly winds are very common in April and May. The highest part of the county is 378 feet above the level of the sea." There are three marked divisions of the county—the Chalk, the Clay, and the Fens, which are each characterised by their flora. An appendix to the work contains remarks on certain obscure species, a list of plants found on the Fens, a list of lost plants, and notes on the geographical range of the plants.

Flora of Suffolk; a Catalogue of the Plants (indigenous or naturalised) found in a Wild State in the County of Suffolk. By the Rev. J. S. HENSLOW, M.A., Professor of Botany, Rector of Hitcham; and EDMUND SKEPPER, Bury St Edmunds. Simpkin & Marshall, London, 1860. 12mo, pp. 140.

This useful catalogue has been compiled from lists and notes furnished by various persons who have examined the botany of different parts of Suffolk. We are informed that Mr Skepper has the entire merit of having reduced these materials to order, and of having seen them through the press, while Professor Henslow's part in the work has been that of a consulting, but otherwise sleeping partner. The reverend Professor has for many years examined the botany of Suffolk, and he has been instrumental in leading others to follow his example. His efforts in this respect among the young of his parish have been long known. The botanical and horticultural lessons given at Hitcham have done much to improve the intellectual and moral character of the population, and to train their observing powers—leading them from nature up to nature's God.

The Professor remarks, "Having had some years' experience of the advantage of introducing botany as an educational weapon in a humble village school, I can strongly recommend it to all who are interested in raising the intellectual status of our village children. Whoever may be desirous of seeing the plan we have adopted at Hitcham, will find the children at their botanical exercises every Monday at three o'clock. I have gradually accumulated sundry memoranda, which I hope to find leisure before long to throw into shape, that others may be saved the mistakes I have made, and profit by the experience I have acquired." The catalogue embraces both the flowering and the flowerless plants of the county; the former being arranged according to Bentham's "Handbook of the British Flora." The number of flowering plants and ferns noticed is about 980.

Notices of recent Botanical Works.

Among botanical works recently published, or in the course of publication, we may notice the various colonial Floras; these have been undertaken by botanists of the highest eminence. They are most valuable contributions to science, more especially to botanical geography. To the Colonies such works are of great importance, not merely by adding to the knowledge of their floral productions, but also by communicating information relative to medicinal and economical plants. In the *Flora Capensis*, by Professor Harvey and Dr Sonder, we have the commencement of a systematic description of the plants of Cape Colony, Caffraria, and Port Natal; while, in Harvey's *Thesauras Capensis*, there are figures and brief descriptions of South African plants, selected from the Dublin University Herbarium. Thwaites, the talented superintendent of the Botanic Garden at Peradenia, is publishing an *Enumeratio Plantarum Zeylaniæ*, or enumeration of Ceylon plants, with descriptions of the new and little known genera and species, and observations on their habitats, uses, and native names. In this work he is assisted by Dr Hooker. We wish much that the excellent *Flora Indica* undertaken by Hooker and Thomson could be completed. The materials are ready, and it only wants the patronage of Government to secure its publication, which would be a great boon to India. Now that the Colonial Office and Colonial Legislatures are aiding in the Floras of our colonies, we may surely entertain a hope that the vegetable productions of our Indian possessions will not be neglected. The *Flora of the British West India Islands* has been undertaken by Dr A. H. R. Grisebach, Professor of Botany in the University of Göttingen; and the *Flora Hong-kongensis*, a description of the flowering plants and ferns of the island of Hong-Kong, has just been completed by Mr Bentham. This little island, situated at the mouth of the Canton river, possesses a singularly rich and varied flora. The island is a rugged mountain ridge, running from east to west, having peaks which attain the height of 1700 to 1800 feet.

The Australian Flora is being illustrated by Dr Ferdinand Mueller, the able and indefatigable superintendent of the Melbourne Botanic Garden. One of his works is entitled *Fragmenta Phytographiæ Australiæ;* another is *An Account of the Plants indigenous to this Colony of Victoria*, with illustrations. The Floras of the Antarctic Islands, of Tasmania, and of New Zealand, have already been given to the world in the splendid works of Dr Hooker.

While the *Botany of the Northern United States* has been published by Professor Asa Grey, that of the *Southern United States* has been recently drawn up by Dr A. W. Chapman, who has described the flowering plants and ferns of Tennessee, North and South Carolina, Georgia, Alabama, Mississippi, and Florida.

The successful establishment of a Botanical Society in Kingston, Canada, will, it is hoped, lead to the publication of a Canadian Flora by Dr G. Lawson, Professor of Natural History in the University of Kingston.

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Monday, 17*th December* 1860.—PROFESSOR KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. Contributions to the Natural History of Volcanic Phenomena and Products in Iceland. By W. Lauder Lindsay, M.D., F.L.S. Communicated by Professor Balfour.

(See Philosophical Journal for January 1861, p. 6.)

2. On the Pediculi infesting the Different Races of Man. By Andrew Murray.

The object of this paper was to determine whether the pediculi infesting the different races of man were of the same or of distinct species, and thereby to ascertain whether any inference could be drawn therefrom, bearing upon the disputed question of the unity of the human species.

The author had obtained specimens of pediculi taken from sixteen or eighteen different races of mankind, and had discovered certain minute differences which appeared to be constant in the different races. These differences, however, did not appear so different in particulars, or marked in degree, as to justify their being considered different species.

Monday, 7*th January* 1861.—PROFESSOR CHRISTISON, V.P., in the Chair.

The following Communications were read:—

1. Note on Hydrated Sulphuric Acid. By Dr Lyon Playfair, C.B.

It is well known that hydrated sulphuric acid loses sulphuric anhydrid on heating, and that its specific gravity decreases in consequence. It is of some practical importance to know the exact circumstances under which this takes place, and the following experiments may prove useful in this respect. Sometimes, in distilling and subsequently evaporating sulphuric acid, I obtained a hydrate of sp. gr. 1·848, and at other times not exceeding 1·842. To explain these differences I took—

1. Monohydrated sulphuric acid, having the sp. gr. 1·848, and containing by alcametrical testing 81·62 per cent. of anhydrous acid, and buried the retort containing it in hot sand, and distilled. The distillate now contained only 80·12 anhydrous acid, and had a sp. gr. of 1·840 at 60° Fahr. It had lost in distillation about 1½ per cent. of anhydrid.

2. The weak acid thus got by distillation was exposed to a temperature near to, but not exceeding, 554° Fahr., for forty minutes. On cooling, its alcametrical strength was 81·615 of anhydrid, and its specific gravity 1·84798.

3. A portion of the first acid, having the strength 81·62 of anhydrid, and the specific gravity 1·848, was boiled violently for two hours. On testing, it was now found to contain only 80·01 of anhydrid and to have a specific gravity of 1·838.

4. The weak acid got in the last experiment was kept for one hour at a temperature of 550° Fahr. The concentrated acid thus got gave 81·62 anhydrid and a specific gravity of 1 84792.

From these experiments, it follows, notwithstanding Marignac's researches, that there is a real monohydrated sulphuric acid obtainable by evaporation, and that its specific gravity is in reality as high as 1·848, which is the number formerly given in books; but that this hydrate is decomposed between the temperature of 550° Fahr. and the boiling point of oil of vitriol.

2. Note respecting Ampère's Experiment for showing the Repulsion of a Rectilinear Electrical Current on itself. By Principal Forbes, LL.D., &c.

The experiment referred to was performed in 1823 by Ampère in the presence of Mr A. De la Rive. It is intended to show that if one portion of a rectilinear conductor is made moveable, it will be repelled from the portions which form its continuation. The arrangement employed was a copper wire-float in a double trough of mercury, which cannot easily be explained without a figure.

This experiment, though quoted by most writers on electricity, is understood to be one not easily repeated. Indeed, the author has not found an indication of its ever having been repeated with success. The author conceiving that perhaps this repulsion described by Ampère might assist in explaining the vibrations of rocking bodies through which an electrical current passes (as described in a communication by him to this Society in January 1859), devised another, and, as he conceives, a more delicate method of testing the supposed repulsion of a rectilinear current on itself. This he accomplished by connecting the two poles of a battery by means of a piece of copper-wire shaped like a horse-shoe, and laid upon one extremity of a slender wooden lever suspended by a fine platinum wire, after the manner of a torsion balance. Yet, on the establishment of the circuit through the bent wire, so far was there from being any repulsive force tending to separate the torsion rod from the battery poles, that the connecting horse-shoe was sensibly and even powerfully attracted to the poles, both while the current lasted, and for some time after.

This experiment seems to throw a doubt upon Ampère's conclusion. The author believes that his mode of experimenting is not only much more delicate than that of Ampère, but also that it avoids sources of ambiguity present in the other.

3. Fragmentary Notes on the Generative Organs of some Cartilaginous Fishes. By John Davy, M.D., F.R.S. Lond. and Edin., &c.

These notes, it is stated by the author, were made at different times and places, as opportunity favoured, and are offered as a contribution to a difficult branch of Ichthyology.

The generative organs of nine different species are described, with more or less minuteness of detail. The species are *Squalus squatina*, *S. galeus*, *S. acanthias*, *S. carcharias*, *S. centrina*, *S. canicula*, *Scyllium melanostomum*, *Raia aquila*, and *R. oxyrhynchus*.

From the observations made, these species would appear to differ in some essential points as regards the reproductive process; some, as the *S. squatina*, being viviparous,—their ova developed or hatched in the uterine cavity; others, as *S. acanthias* and *S. carcharias*, being oviporous, their ova included in a shell with a white, yet developed in the same cavity, the presence of a shell constituting the difference comparing them with the preceding; others, as *R. aquila*, and *S. canicula*, being oviparous, their eggs experiencing no embryonic development in the oviducts, and hatched after exclusion in the sea.

Respecting the anal appendages, the characteristic of the male cartilaginous fish, the opinion the author is most inclined to adopt is, that they are not claspers, but rather organs of intromission; preferring this latter—the old view of their use, as noticed by Aristotlé—to the former, which is the more recently entertained one, from considering their structure and certain facts which have come to his knowledge.

Concerning the branchial filaments, which have commonly been held to be mainly subservient to the supplying with air the blood of the embryo, he thinks they may have another use also, that of aiding the growth of the parts to which they belong. Their early absorption, and being sometimes found in other regions of the embryo than the branchia, are brought forward as circumstances favourable to this idea.

Monday, 21*st January* 1861.—The Very Reverend DEAN RAMSAY, V.P., in the Chair.

The following Communications were read:—

1. On a Method of taking Vapour Densities at Low Temperatures. By Dr Lyon Playfair, C.B., F.R.S., and J. A. Wanklyn, F.R.S.E.

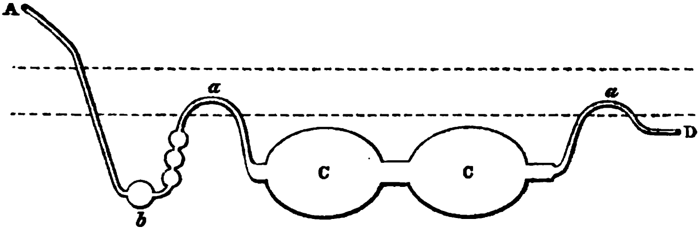
The authors refer to Regnault's experiments, which have shown that aqueous vapour in the atmosphere has the same vapour density at ordinary temperatures as aqueous vapour above 100° C.; and they bring forward fresh experiments upon alcohol and ether to show that when mixed with hydrogen these vapors preserve their normal density at 20° or 30° C. below the boiling points of the liquids, and infer generally that vapours, when partially saturating a permanent gas, retain their normal densities at low temperatures.

From their researches the authors deduce the consequence—remarkable, but quite in harmony with theory, that permanent gases have the property of rendering vapour truly gaseous. Stated in more precise terms, the proposition maintained by the authors is, "The presence of a permanent gas affects a vapour, so that its expansion-coefficient at temperatures near its point of liquefaction tends to approximate to its expansion-coefficient at the highest temperatures."

The authors anticipate that admixture with a permanent gas may serve as a kind of re-agent to distinguish between cases of unusually high expansion-coefficient in a vapour, and cases where chemical alteration takes place. It will also be possible, by the employment of a permanent gas, to obtain vapour-densities of compounds which will not bear boiling without undergoing decomposition.

In experimenting upon substances which may be heated above the boiling point, the authors employ Gay Lussac's process for taking the specific gravity of vapours. A slight modification is, however, necessary. Previous to the introduction of the bulb containing the weighed substance, dry hydrogen is introduced into the graduated tube and measured with all the precautions belonging to a gas analysis. It will be obvious that in the subsequent calculation the volume of hydrogen corrected at standard temperature and pressure must be subtracted from the volume of mixed gas and vapour, also corrected at standard temperature and pressure.

When the substance will not bear heating to its boiling point, the authors employ a process resembling that of Dumas in principle, but differing very widely from it in detail. Dumas' flask with drawn-out neck is replaced by two bulbs, together of about 300 cub. cent. capacity, joined by a neck, and terminating on either side in a narrow tube. One of the narrow tubes has some very small dilatations blown upon it (*b*), the other is merely bent (D). (See the drawing.) The apparatus, whose weight should not exceed 70 grm., is weighed in dry air, then placed in a bath, being secured by a retort-holder grasping the neck joining the large bulbs C and C. The end A, projecting over the one side of the bath, is made to communicate with a hydrogen apparatus; the end D passes through a hole in the opposite side of the bath, which is plugged up water-tight by means of putty. Dry hydrogen is transmitted through the whole arrangement, and escapes at D through a long narrow tube joined to it by a caoutchouc connecter.



The bath is next filled with warm water until the bends *a* and *a* are covered. The connection with the hydrogen apparatus is then for a moment interrupted, to allow of the introduction of a small quantity of the substance at A. The substance, which should not more than half-fill the small bulb *b*, is partially vaporised in the stream of hydrogen, and in that state passes into the part CC. All the while, the temperature of the bath is kept uniform throughout by constant stirring, and made to rise very slowly. When within a few degrees of the temperature at which the determination is to be made, the current of hydrogen is almost stopped, so that the bulbs C and C may contain less vapour than will fully saturate the gas at the temperature of sealing. The water of the bath is then made to subside, by opening a large tap placed near the bottom. The bends *a* and *a* are thus exposed, the bulbs CC remaining covered. Immediately the current of hydrogen having been stopped, the flame is applied at *aa*, so as to seal the apparatus hermetically. The temperature of the bath, as well as the height of the barometer, must now be observed. After being cleaned, the apparatus (which now consists of three portions, viz., the portion CC hermetically sealed and the two ends *b* and D) must be weighed.

The capacity of the apparatus is found by filling it completely with water and weighing; but previously to this operation the volume of hydrogen enclosed at the time of sealing must be found. On breaking one extremity under water, the water will rise in the bulbs, and, after a while, will have absorbed all of the vapour, but will leave the hydrogen. The bulbs must then be lifted out of the water, without altering their temperature, and, with the water that has entered, weighed. The difference between the latter weighing and the weight of the bulbs quite full of water gives the weight in grammes, which expresses in cubic centimeters the volume of hydrogen enclosed; the pressure is the height of the barometer minus the column of water which had entered the bulbs; the temperature is that of the water.

An example of a determination of the vapour density of alcohol at 30° C. below its boiling point is subjoined :—

Height of the barometer (at 0° C.), .	763·09 m.m.
Temperature of the balance case, . .	7·5° C.
Weight of apparatus in dry air, . .	69·959 grm.
Temperature at time of sealing, . .	48° C.
Weight of apparatus + hydrogen + vapour,	69·5275 grm.
Weight of apparatus + water (at 5.2° C.)	191·76 grm.
Weight of apparatus filled with water, .	545·36 grm.
Height of water column, . . .	122 m.m.

From which is deduced :—

	Volumes corrected at 0° C. and 760 m.m. pressure. Cubic centimeters.		Grm.
Hydrogen + vapour,	406·43	weighing	0.1695
Hydrogen,	341·27	,,	0·0306
	65·16		0·1389

Therefore, 65·16 cub. c. of alcohol vapour weigh ·1389 grm.
but 65·16 cub. c. of air weigh ·0843 grm.

$$\text{Vapour density of alcohol} = \frac{\cdot1389}{\cdot0843} = 1\cdot648$$

The authors have extended their experiments to acetic acid and other substances. At low temperatures the vapour-density of acetic acid approximates to 4·00, no matter how much hydrogen be employed. At higher temperatures, an approximation to 2·00 is obtained, but without heating so high as Cahours found necessary.

The authors are continuing these researches.

2. Memoir of Sir Thomas Makdougall Brisbane. By Alexander Bryson.

Monday 4th February 1861.—Principal FORBES, V.P., in the Chair.

The following Communications were read :—

1. Notes on the Snow Crystals observed during the late Frost. By Professor Allman.

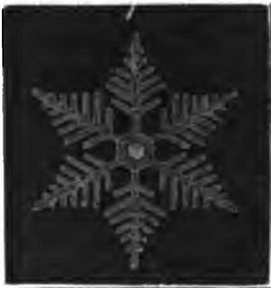
On the 26th of December last, about half-past 11 A.M., a light snow shower fell in Edinburgh, and lasted about half an hour. The air was at the time still, and the sky overcast with a thin haze, while the thermometer stood at many degrees below the freezing point. The appearance of the snow was very remarkable. It fell in loose open flakes, and lay upon the surrounding objects in little masses like tufts of exquisitely white, soft and light down.

Remembering the descriptions of the crystals of snow in high arctic regions, and during intense frost even in our own latitudes, as given by Scoresby, Glaisher and others, I was desirous of determining how far the structure of the snow now falling corresponded with the accounts of these observers. I accordingly, with the view of rendering their composition more apparent by contrast with a dark surface, collected a few of the falling flakes upon a sheet of blackened pasteboard, when, even to the unassisted eye, a structure of marvellous beauty was at once revealed. The white down-like snow-flake was now seen to be an aggregate of symmetrical and transparent ice stars, many of them more than a quarter of an inch in diameter. When examined under the compound microscope with the aid of a two-inch object-glass, their beauty became still further enhanced, and it was then seen that every star was itself composed of a multitude of transparent crystals, in some cases tabular, in some acicular, and all grouped in obedience to a definite law, so as in their wonderful assemblages to give rise to shapes of exquisite symmetry,—shapes, too, of almost infinite variety; the kaleidoscope, in its magical transformations, is not more rich in forms,—yet all pervaded by an unbroken unity, for their type had been already fixed; and even in their most sportive mood, they could be seen to be under the control of a definite number and a definite quantity—the number 6 and the angle of 60° determining their form and limiting their variations.

Among the various figures which I observed on the occasion referred to, we may perhaps select, as the archetypal combination, a 6-rayed star (fig. 1), having for its centre or nucleus a tabular crystal in the form of a regular hexagon, each of whose angles supports one of the six rays of the star. Each of these rays would seem to be a very much elongated hexagonal prism, and every one of them gives off from each side, and in the common plane of the figure, secondary arms, which spring from it with a pinnate arrangement, and at an angle of 60°. These secondary arms in the figure under consideration are also elongated hexagonal prisms; in the greater number of instances, however (figs. 2–5), the secondary arms are wedge-shaped, and probably the result of more complex conditions than those under which the type form is produced. The axis of symmetry of each of these wedge-shaped arms is inclined to the ray at an angle of 60°, but I cannot say at what angle the sides of the arms are inclined to one another. The central hexagon in the figure, from which the sketch was taken, was marked with elegant concentric striæ.

It is here worth noting, that the figure I have assumed as the type consists exclusively (if we except the termination of the arms) of a combination of the two limiting forms of the Rhombohedron mR; namely, that in which m becomes practically 0, and that in which it becomes infinite, producing in the former case the tabular crystal $0R$, which constitutes the centre of the figure, and in the latter case the prismatic crystals ∞R, which constitute the rays and their branches.

Assuming then the form now described as the type, there would seem to be but little difficulty in deducing from it most of the peculiarities presented by the other figures which I have had an opportunity of observing. In fig. 2, the central hexagon has disappeared, and the rays meet at a point in the centre of the star.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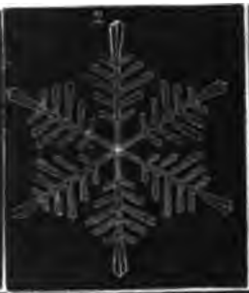
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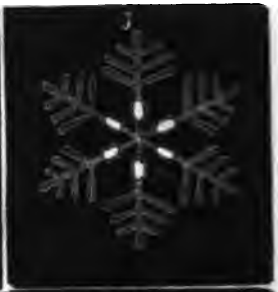
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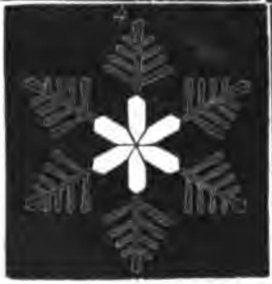
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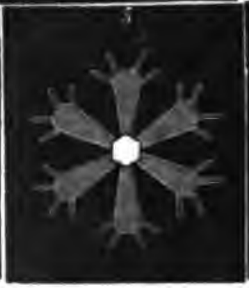
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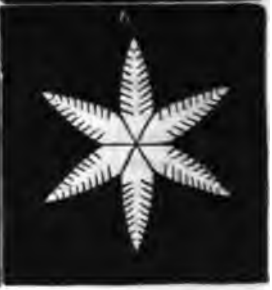
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In fig. 3, a tabular hexagonal crystal is developed diagonally in the course of each ray at a uniform distance from the centre, and in the common plane of the star.

Fig. 4 is easily derived from fig. 3; for in order to produce it, we have only to suppose all the six hexagons to be simultaneously prolonged in the direction of the ray, until they meet in the centre of the figure, when, in consequence of mutual and symmetrical interference, they will terminate in an angle of 60°, or of half the proper angle of the hexagon, thus forming *maccles* whose plane of union is inclined to the principal axis of the ray at an angle of 30°.

In fig. 5 we have a central hexagon, carrying symmetrically on each of its angles a wedge-shaped five-sided table, which thus takes the place of the acicular ray in fig. 1. Three of the sides of this table appear to be those of the regular hexagon, while the remaining two sides would, if continued, meet at an angle which I have not been able to measure, somewhere within the central hexagon. From each of the free angles of the wedge-shaped ray, a little hexagonal prism is developed.

The simply pinnate rays of fig. 2 occasionally become doubly pinnate. I have found even trebly pinnate varieties.

I also noticed a form (fig. 6) which is not exactly traceable to the *type*, and which I am inclined to view rather as an abnormal condition induced upon one of the other forms by the coalescence, or imperfect development, of the secondary arms. It consisted of a six-rayed star, in which the rays, instead of being prismatic or wedge-shaped, were lanceolate, the curved boundaries thus bringing this modification almost entirely into the type of organic form. The edges of the rays were marked with pinnate serrations or striæ.

In this last form we are strongly reminded of the beautiful stellate figures, like six-petalled flowers, which have been recently described by Professor Tyndal as being developed within a block of ice, when the rays of the sun are concentrated on a point in the interior of it by a burning lens.

Besides the forms now described, the snow which fell on the 26th contained many others, any one of which would well repay the trouble of drawing, though no figure can give an adequate idea of the elegance of the actual object, in which the most beautifully symmetrical flowers, and elaborately divided fern leaves, are repeated with that marvellous fidelity with which inorganic nature occasionally imitates the forms of organisation when the formative forces are directed by an undeviating symmetry.

But the ordinary snow of our latitude is not thus constituted. We may seek in vain in the snow as it usually falls with us for the beautiful stellate crystalline groups now described. Why is this? There are doubtless needed, for the perfect development of the ice crystals in the freezing cloud which is about to descend in the form of snow, several conditions, as yet but partially understood. Of these, it is possible that a temperature considerably below the freezing point may be one, and a stillness in the higher regions of the atmosphere, where the snow is formed, another. It is exceedingly probable, as has been ingeniously suggested by Fr. Vogel,* in explanation of the phenomena of hail, that if the atmosphere be perfectly still, the particles of visible vapour constituting the cloud may be cooled far below the freezing point without freezing; and if in this state a crystal of ice be precipitated into the cloud from some higher elevation, or the stratum of cold vapour be agitated by sudden exposure to some atmospheric current, the balance between the forces of cohesion and adhesion will be destroyed, and the whole cloud will instantly shoot into beautiful ice-crystals, and will fall towards the earth as stellate snow; for the peculiar constitution of the visible vapour is such, that unimpeded action will be permitted to the forces of crystallisation, and no mechanical obstacle will be offered to the perfectly symmetrical development of the crystalline groups.

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* See Müller's Kosmische Physik, p. 421.

If the temperature of the inferior strata of the atmosphere be sufficiently low, the snow-crystals will reach the earth unaltered; but if, in their descent, they happen to pass through strata whose temperature is above the freezing point, they will—if they be not actually converted into rain—lose the sharpness and beauty of their outline, while partial thawing, followed, at the surfaces of contact, by the singular phenomenon of regelation, which, as has been shown by Faraday, may take place in an atmosphere considerably above the freezing point, will probably contribute to their irregular conglomeration into an ordinary snow-flake, contrasting as this does so strongly with the light, open, down-like flake produced under circumstances favourable to the perfect development and persistence of the crystals.

On the 27th, the snow which had fallen during the preceding day was still lying on the ground, and, with the view of continuing my observations, I again placed some of it under the microscope. I now, however, found that the crystals had lost all their beauty, and no longer presented the sharpness of outline and symmetry of form which had so forcibly struck me the day before. And yet, during the interval, the thermometer had never risen to the freezing point, nor had the snow been exposed to the direct action of the sun. The change of form thus undergone by the crystals appears to me to admit of but one explanation, and is evidently due to the partial dissipation of the crystal by evaporation, thus affording an interesting example of the evaporability of ice at temperatures considerably below the freezing point.

While on this subject, I may as well mention another fact illustrative of the same phenomenon.

It will be remembered that on more than one occasion during the severe frost a dense fog settled over the city, and was afterwards condensed and frozen on the surrounding objects, covering everything on which it lay, but especially the naked branches of the trees, with the most exquisite frostwork. This beautiful phenomenon, however, was but of short duration; for in less than twenty-four hours, though the temperature continued all the while below the freezing point, and the air free from wind, which might have shaken the frozen particles from the trees, yet not a trace of the frostwork remained.

To form, then, a true conception of the constitution of a snow-flake produced under the conditions which prevailed during the late intense frost, we have only to imagine thousands of stellate groups, such as those just described, entangled together by their complex arms into little loose flocculent masses. The peculiar light down-like character of the snow which fell on the 26th of December is thus easily understood.

On the 7th of January, between 4 and 5 P.M., the sky overcast, and the thermometer standing considerably below the freezing point, Edinburgh was visited by another snow shower. On this occasion the shower was of a very peculiar kind, for the stellate groups of crystals, instead of being aggregated into flakes, as in the shower of the 26th December, were for the most part isolated and distinct, so that the whole atmosphere was filled with separate ice-stars in inconceivable multitudes, whirling through the air, and drifting past one another in mazy paths which no eye could follow, so endless were their intersections, and inextricable their labyrinths; the wonderful ice-drift heaping itself up like white sand on the surrounding objects, but when projected against dark surfaces, revealing itself in all the unrivalled symmetry and beauty of its starry atoms. The chief difference between the present snow shower and that which fell on the 26th December, consisted in the segregation of its crystalline particles. There was a brisk breeze stirring at the time, in the lower regions of the atmosphere, and this was doubtless the invisible analyst that broke up the snow-flakes into their component elements, and flung them in stars and flowers to the earth.

The following Note from Mr Stephens to Dr Balfour was also read:—

REDBRAE COTTAGE, 14*th January* 1861.

MY DEAR SIR,—In December 1859, I was visiting Sir Robert Peel at Drayton Manor, in Staffordshire; and while there, a very heavy hoar-frost occurred for nearly two days,—every tree and shrub and the grass was fringed with most beautiful silver filagree. When walking along the shrubbery in the forenoon, I was led, from the remarkable exuberance of the hoar-frost, to examine it more minutely than by a casual glance; and to my surprise and delight, I perceived that the crystals presented quite a different arrangement on different bushes of evergreens. This there was no doubt of, as I could easily discern the difference in the arrangement of the crystals by the help of a large magnifying glass which I always carry with me when I go from home. I regret I did not take a sketch of the different arrangement of the crystals; but not being prepared for such a proceeding, and being no hand at sketching, I contented myself with looking from one bush to another. And while thus engaged for some time, I thought I made an interesting discovery—namely, that while different bushes presented a different form of crystallisation, the same sort of bush presented the same form of crystals. This discovery interested me very much—for discovery I deemed it, not having observed any mention of such a thing in any book on meteorology I have seen. Thus, Portugal laurels, laurel bays, laurustinas, different sorts of Arbor-vitæ, the yew, some of the new pines, presented different forms of crystals, but the same kind of plant had the same form of crystal. This I made sure of by repeated observation in different parts of the shrubbery; and more than that, the size of the crystals was about in proportion to the size of the leaves of the evergreens. Thus, the crystals in the Portugal laurel, laurel bays, and larger leaved kinds of evergreens, were larger than on the leaves of the laurustina, and these latter larger than in those of the yew and Arbor-vitæ. The crystals, moreover, were not spread over the surface of the leaves, but only along their margins; and the leaves above, that were exposed fully to the air, had the crystals—those within the bush, or under shelter of the leaves above them, were free of all hoar-frost. I may mention that the sun was obscured the first day by a frost-fog; the second day was clear, but the crystals retained their form where the sun did not strike; and on the following morning, the whole fairy scene had disappeared. Tall grasses and sedges by the side of a lake presented different crystals, but the same kind of plant similar forms. The naked branches of trees presented similar results. . . . .

I am, &c.,

HENRY STEPHENS.

2. The Bifilar Magnetometer: its Errors and Corrections. By John Allan Broun, F.R.S., Director of the Trevandrum Observatory. Communicated by Professor Tait.

3. On the Horizontal Force of the Earth's Magnetism. By John Allan Broun, F.R.S., Director of the Observatory of the Rajah of Travancore. Communicated by Professor Tait.

The conclusions of this paper are derived from observations made at Makerstoun in Scotland; Toronto, Canada; Trevandrum, Travancore, and Singapore, East Indies; St Helena, the Cape of Good Hope, Hobarton, Van Dieman's Island, &c. The author has corrected and discussed all the observations published made in the colonial observatories.

The observations have been found affected by different errors, and frequently the series have been broken. An attempt has been made to correct these series; and, in general, to render the series as near the truth as possible.

4. Notice of an Instrument intended for the Measurement of Small Variations of Gravity. By John Allan Broun. Communicated by Professor Tait.

5. On the Law of Growth in Woody Circles of Exogenous Trees, as indicated by the Examination of a Single Specimen. By Principal Forbes.

Monday, 18*th February* 1861.—The HON. LORD NEAVES, V.P., in the Chair.

The following Communications were read:—

1. On the Embryology of Asteracanthion violaceus (Linn. Sp.) By Wyville Thomson, LL.D., F.R.S.E., M.R.I.A., F.G.S., &c., Professor of Natural History in Queen's College, Belfast.

From the author's observations, it appears that the first step in the development of this form of echinoderm embryo is the differentiation of a portion of the yolk into an investing layer of structureless "Sarcode;" that the layer gradually increases in thickness; and that finally, from one part of its surface, a branched peduncular process is produced, as an extension of the same transparent structureless material. The branches of this organ are terminated by suckers, and serve, among other functions, as organs of locomotion. When fully formed, they are undistinguishable in structure and function from the ambulacral feet of the star-fish; a fluid undistinguishable from the chylaqueous fluid of the ambulacral system moves in them with the same characteristic motion. The peduncle is closed externally, no communication, except by transudation, existing between its cavity and the surrounding medium. At first it communicates with the general cavity of the embryo, but afterwards it becomes connected with, and part of, the ambulacral circulating system. When the ambulacral vessels and suckers of the young star-fish become fully developed, this provisional vascular tuft disappears, leaving no apparent scar. In the species described, the peduncle is not connected in any way with the madreporic tubercle, which is not developed till long after its disappearance, and then on the opposite surface of the body.

The peduncular appendage seems to be essentially a provisional development of the ambulacral vascular system, and to be functionally analogous to the omphalo-mesenteric and umbilical vessels of the vertebrate groups, endowed, however, with a greater amount of versatility of function, corresponding to that of the peculiar vascular system of which it forms a part, and to a great degree dependent upon the peculiar vital properties of the substance entering into its structure.

2 Some Observations on the Albino. By John Davy, M.D., F.R.S. Lond. and Ed., &c.

The subjects of these observations were five individuals—four females, one male—all natives of Ceylon, of its south-east salubrious coast, and the children of coloured parents.

They are described by the author as well made, as having good health, and, though all coming under the denomination of Albinos, on account of their unusual fairness, yet as varying in degree, the lightest having red eyes and almost white hair; the less light, blue eyes and light-brown hair, with a complexion that in this country would be considered merely that of a pure blonde.

It would appear that the Albino in Ceylon, contrary to what has been asserted, is not held in contempt, but rather in respect, in accordance with the feeling of the people towards persons of high caste, who are almost invariably of a lighter hue than the more laborious and exposed low caste.

A speculation of the Singalese is mentioned relative to the white races, viz., that they are the descendants of Albinos, and, *ab origine*, merely an accidental variety.

The author seems to think that analogy is not unfavourable to this hypothesis, keeping in mind how great is the variety of colour, and its gradations from light to dark, amongst Europeans, and, in the instance of our domesticated animals, how colour is hereditary; and also on the ground that the sun's rays have a greater darkening effect on persons of brown skin than on those of fair; and that the former, especially the darkest—the blacks—are better able to resist malaria and the effects of tropical climates than the whites, and have thereby a better chance of escaping disease, and a premature death and extinction of race.

3. Note on the Bisulphide of Iodine. By Frederick Guthrie.

4. Notice of an Expeditious Method (believed to be new) for Computing the Time of Descent in a Circular Arc. By Edward Sang, Esq.

5. Note on a Modification of the Apparatus employed for one of Ampère's Fundamental Experiments in Electrodynamics. By Professor Tait.

My attention was recalled by Principal Forbes's note (read to the Royal Society on January 7th), to his request that I should at leisure try to repeat Ampère's experiment for the mutual repulsion of two parts of the same straight conductor, by means of an apparatus which he had procured for the Natural Philosophy Collection in the University. Some days later I tried the experiment, but found that, on account of the narrowness of the troughs of mercury, it was impossible to prevent the capillary forces from driving the floating wire to the sides of the vessel. I therefore constructed an apparatus in which the troughs were two inches wide, the arms of the float being also at that distance apart. Making the experiment according to Ampère's method with this arrangement, I found one small Grove's cell sufficient to produce a steady motion of the float from the poles of the pile; in fact, the only difficulty in repeating the experiment lies in obtaining a perfectly clean mercurial surface.

Two objections have been raised against Ampère's interpretation of this experiment, one of which is intimately connected with the subject of Principal Forbes's note. This is the difficulty of ascertaining exactly what takes place where a voltaic current passes from one conducting body to another of different material. It is known that thermal and thermo-electric effects generally accompany such a passage. To get rid of this source of uncertainty, I have repeated Ampère's experiment in a form which excludes it entirely. In this form of the experiment the polar conductors and the float form one continuous metallic mass with the mercury in the troughs; the float being formed of glass tube filled with mercury, with its extremities slightly curved downwards so as nearly to dip under the surface of the fluid; and the wires from the battery being plunged into the upturned outward extremities of two glass tubes, which are pushed through the ends of the troughs so as to project an inch or two inwards under the surface of the mercury. A little practice is requisite to success in filling the float and immersing it in the troughs without admitting a bubble of air. This float, being heavier than the ordinary copper wire, plunges deeper in the fluid, and encounters more resistance to its motion, but, with two small Grove's cells only, Ampère's result was easily reproduced, even when the extremities of the float rested in contact with those of the polar tubes before the circuit was completed. It is obvious that here no thermo-electric effects can be produced in the mercury, and I have satisfied myself that the motion commences before the passage of the current can have sensibly heated the fluid in the tubes.

The other class of objections to Ampère's conclusion from this experiment, depending on the spreading of the current in the mercury of the troughs, is of course not met by this modification. I have made several experiments with a view to obviate this also, but my time has been so much occupied that I have not been able as yet to put them in a form suitable for communication to this Society.

Royal Physical Society.

Wednesday 28th November.—William Rhind, Esq., President in the Chair.

The President delivered an Opening Address.

The following communications were read:—

1. *Observations on British Zoophytes and Protozoa.* (1.) *Notice of* Ophryodendron abietina (Corethria sertulariæ). (2.) *On the Reproductive System of* Chrysaora. By T. Strethill Wright, M.D.

(1.) On *Ophryodendron abietina.*—The author stated that, amongst the lower classes of animals, and especially in the *Protozoa*, the lowest class, numerous and very striking examples of homomorphism occurred. "Homomorphism" was an exact similarity in form between animals of different classes, without any corresponding resemblance in their anatomical structure. Some of these examples might be considered fanciful, as the likeness between *Lacrymaria olor* and the fossil reptile *Plesiosaurus*. But in others the homomorphism was so perfect, that animals belonging to the lower class had been long confounded by the most eminent zoologists with those of a higher class. Thus, various species of the *Foramenifera* had been classed amongst the *Cephalopoda*. The shells of many of the *Foramenifera* were, indeed, exact copies of those of *Cephalopoda*, both recent and fossil. The recent Nautilus and Argonaut, and the fossil Baculite, Orthoceratite, Hamite, and Ammonite, found their representatives in the microscopic Numulina, Polystomella, Dentalia, Cristellaria, and Rotalina. The shells of the former were inhabited by the highly organised cuttle-fishes; those of the latter by creatures which could scarcely be said to possess any organization. The chambers of their shells were filled with a glairy living mass, which streamed like a fluid in and out through the innumerable minute pores with which the shells were pierced. The streams united together to form widely-spread meshes and expansions, which enveloped, absorbed, and digested smaller living beings coming in contact with them, and on which the animals moved, or rather flowed along. But although the forameniferous animal was a mere fluid mass, destitute not only of organs and stomach, but even of the simplest cellular structure, it was yet capable of exercising the most important functions of life, motion, nutrition, and reproduction, and of erecting for itself edifices mathematically correct in design, which arrested the eye by their exceeding beauty of form and ornamentation, and which, deserted by their tenants through successive ages, had formed no inconsiderable part of the solid framework of our globe. A curious instance of "homomorphism" occurred in the subject of the present notice, *Ophryodendron abietina*, which was fashioned after the type of *Sipunculus Bernhardi*, a highly organised Echinoderm. The animal consisted of a shapeless oblong mass, immoveably fixed to the corollum of *Sertularia pumila*. From one end of the mass arose a closely wrinkled proboscis, surmounted by a tuft of short tentacles. The proboscis could be entirely withdrawn into the body, or extended to an astonishing length, until it appeared as a clear glassy wand, thirty times as long as the animal, and clothed at its upper part by about forty scattered tentacles, which twined about in most violent motion. The animal seemed to be constantly searching the water around for prey, and occasionally to press the tentacles firmly against the body of the proboscis, as if to imbed some matter into the soft substance of the latter—the usual mode of feeding amongst the *Acinetiens*, to which class it belonged. It was impossible not to be struck by the extreme similarity in outward form between this animal and the Echinoderm *Sipunculus Bernhardi*. In both animals occurred the same shapeless body, the same entirely retractile proboscis crowned with tentacles, and the same peculiar motions in seeking for prey. But with the form the similarity ended, for the Echinoderm possessed a highly organised structure, while in the transparent Protozoon no structure at all had been detected.

(2.) On *Chrysaora hyoscella*.—The author stated that, while other Acalephæ were bisexual, *Chrysaora hyoscella* was hermaphrodite. The sperm sacs were found on peculiar transparent grape-like bodies, and on tentacles, both springing from the ovarian membrane. Sperm sacs were also discovered on tubercles, attached to various parts of the lining membrane of the stomach and oral tentacles or lips, down to the very tips of the latter organs.

2. *On the Articular Processes of the* Atlas *and* Axis. By JOHN CLELAND, M.D.

3. *A Gyrfalcon* (Falco Islandicus, *Lath.*), *shot in Uist; and a Great Spotted Woodpecker* (Picus major, *Linn.*), *killed in the neighbourhood of Edinburgh, were exhibited by* JOHN ALEXANDER SMITH, M.D.

Dr JOHN ALEXANDER SMITH exhibited a very large and fine specimen of a cup-shaped sponge, *Halichondria ventilabrum*, from Shetland; it was sent by Mr Carfrae, Princes Street.

Wednesday, 23d January.—ALEXANDER BRYSON, Esq., President, in the Chair.

The following Communications were read:—

1. *On Inflammation in Fishes.* By ALEXANDER M'K. EDWARDS, Esq.

Mr Edwards read the results of some experiments he had made to ascertain the effects of injuries on the tissues of fish. He found that the irritation of setons caused suppuration and ulceration; that raw surfaces healed by granulation, leaving firm depressed cicatrices, and that simple incised wounds healed by primary adhesion, and that the entrance of water between the cut surfaces was prevented by the exudation of a material resembling lymph. It was also remarked, in the course of these experiments, that the fish seemed quite indifferent, both to the infliction of the wounds, and to meddling with the latter afterwards, until the processes of inflammation had set up, when they appeared to feel acutely in the injured parts. Preparations of cicatrices, fractured bones of fish, &c., were exhibited, which had been given to Mr Edwards by Dr J. Sidey, and Mr Moffat, fishmonger, tending to prove that injuries in fish were repaired by methods very similar to those of land animals.

Dr M'BAIN exhibited several specimens of *Halichondria ventilabrum*, *H. infundibuliformis*, and *Tethea Cranium*, from the Shetland Islands. He stated that the fine specimen of *H. ventilabrum* shown at the first meeting of this session, by Mr Carfrae of Princes Street, was caught up by a fisherman's net off Sumburgh Head, the southern point of Shetland. It measured 21½ inches across, and 10 inches in height.

3. *Report of the Committee on Marine Zoology; with a Notice of the Sprat-Fishing in the Firth of Forth* By GEORGE LOGAN, Esq., Convener.

The Committee, among numerous other captures, obtained a fine specimen of *Corystes cassivelaunus*, the masked crab, dredged up in Aberlady Bay. Living specimens of *Kellia suborbicularis*, found plentifully in dead valves of *Tapes virginia*, near Inchkeith, in September last, lived for rather more than six weeks. Although great numbers of the hermit crab, *Pagurus*, were examined, dredged in about six fathoms water, in expectation of finding the parasite, *Peltagaster paguri*, frequently attached to it, not a single specimen was discovered. While this parasite seems thus to be absent in deep water, it is found abundantly attached to the shore crab, and *Peltagaster carcinus* was found at Trinity attached to the abdomen of the common edible crab, sometimes two or three on one animal. One specimen of *Psammobia ferroensis* was found near Inchkeith, and occasionally on old valves were observed the curious pear-shaped, stalked ovarian niduses, of *Pontobdella muricata*, frequently passed over as the undeveloped state of *Himanthalia lorea;* some specimens were also procured of *Nemerts Borlassii*, of a beautiful full dark purple colour, and of enormous length. On the 28th of September four or five specimens of a small flat fish came up in the dredge, which appears to be the *Monichirus linguatulus* (Cuvier), first observed in this country by Parnell on the Devonshire coast, and noticed by him in 1837, in the "Transactions of the Royal Society of Edinburgh," and in the "Magazine of Zoology and Botany," under the name of *Monichirus minutus*. He does not include it in his "Fishes of the Firth of Forth," and the Committee are not aware of its having before been obtained there; but from the occurrence of so many specimens in one day, it would seem to be not very rare. Some of the specimens were brought home alive, and one of them lived for nearly three months, until killed by the recent severe cold. It was very sluggish, remaining always at the bottom of the aquarium, and seldom moving unless disturbed. Certain members of the Committee devoted much time and attention to the herring and sprat or garvie fishery of the Firth of Forth; and the result of close investigation of large masses of fry at various times has been, that the proportion of herring, *Clupea harengus*, less than six inches long, taken among the sprats, *Clupea sprattus*, is very small; one of the examiners found on an average only one herring fry among a hundred sprats over a very extensive field of investigation; others found the proportions of herrings rather larger, but they all concurred in thinking that the comparatively small quantity of herring fry caught with the sprats could not in any sensible degree affect the former; and when it is considered that the roe of a single herring contains such an enormous quantity of ova (32,000), and that the fish spawns upon our shores in countless millions annually, the sensitiveness which has been shown as to the taking of even a few tens of thousands of young herrings among the sprats seems to be without foundation. A limited close-time, or protection in a limited area, would probably do infinitely more towards the protection of the fishery than any needless and unpopular prohibition of sprat or other fishing. It may be useful to mention the prominent and easily noticed markings which separate the two species, so that any one, upon looking over the surface of a mass of fish so abundantly brought to market, may distinguish without difficulty the one from the other. The head of the herring, from the point of the lower jaw to the farthest part of the gill-plates, is about a fourth-part longer than the head of the sprat; the eye is one-third larger; the lower jaw projects more beyond the upper jaw; the tail of the herring is dark; that of the sprat light-coloured, much broader, shorter, and less forked than that of the herring; and the body of the fish at the insertion of the tail is also much broader; the abdominal line is strongly serrated and sharp all along; the same line in the herring is rounder and quite soft (excepting in small specimens, but there is never any serration under the pectoral fins); the sprat is more plump and compact, and resembles in shape a miniature salmon, and the scales are larger, and their insertion much further apart than in the herring. Upon the vexed subject of herring spawn it may be stated, that during several years, while the herring has been upon the coast in abundance, the Committee, from the beginning of August to the middle of October, have dredged and trawled in Aberlady Bay and off North Berwick down to Tyningham Sands, without bringing up a trace of herring spawn; and they are quite satisfied that none is deposited upon proper trawling or dredging ground. Among the millions of fry which have come under the observation of the Committee during the present month, not a single specimen of whitebait, *Clupea alba*, has been detected; and, what is somewhat remarkable, although whitebait was found in abundance at Seafield in the autumn of 1859 without any admixture of sprats, during last season, at the same period, only sprats were found, without any whitebait.

5. *Notice of an apparent Hybrid between the Black Grouse and Red Grouse; shot near Mid-Calder.* By John Alexander Smith, M.D.

Dr John Alexander Smith exhibited a specimen of the *Strix Tengmalmi* (Tengmalm's owl), taken on Cramond Island about the end of the year. Only one or two instances of its capture in England have been recorded, but none in Scotland that Dr S. had observed.

Mr James B. Davies exhibited the following specimens from Dr E. W. Dubuc, R.N. A small *Physalia* (Portuguese man-of-war) from the shore at Kurrachee, Scinde. Specimens of a species of *Pinna*, both dry, and dissected, in spirit, from Trincomalee, Ceylon; two specimens of *Pinnotheres* (a small crab), one of which inhabits the interior of the shell of *Pinna*, and the other from the *Placuna placenta*, also from Ceylon; specimens of the Trepang, or edible *Holothuria*, in spirit, and dried, as for the Chinese market. Regarding these, Dr Dubuc supplies the following notes:—"Whilst at Trincomalee, in the summer of last year, in medical charge of the hospital hulk Sapphire, my attention was drawn to a species of *Holothuria*, very abundant on the sandy bottom of the inner harbour, at a depth of about two to three fathoms. The animal is of a yellowish-brown colour, the skin over the dorsal aspect being tough and coriaceous. The ventral surface presents numerous ambulacra, and is of a pale colour, and close to the anal orifice there is the retractile branchial tuft. At Naples the *Holothuria tabulosa* is employed as an article of diet, and at the Marianne Islands the *Holothuria guamensis*; whilst the Chinese prefer the *Holothuria edulis*, or Trepang, which is common in the Chinese seas, principally at the Anamba Islands, where the Malays fish for it with hooks fastened to the ends of long bamboos."

Botanical Society of Edinburgh.

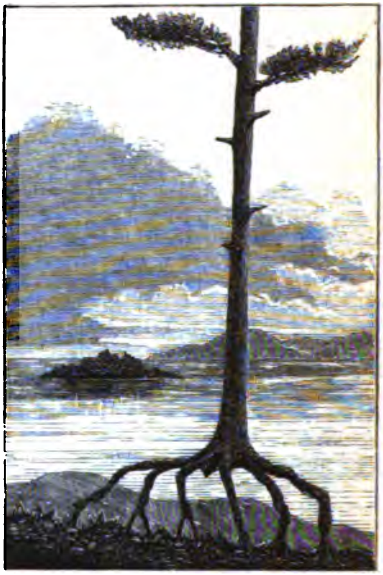
8th November 1860.—Professor Allman, President, in the Chair.

The President delivered an opening address.

The following Communications were read:—

1. *On Certain Peculiarities in the Growth of Pine Trees on the East Shore of Lough Neagh, county Antrim, Ireland.* By the Rev. A. Colquhoun Canning.

Mr Canning remarked:—It is known to naturalists that the extensive sheet of water—Lough Neagh, in Ulster—is connected with interesting peculiarities, both of a geological and botanical kind, which do not seem to have received that close scientific consideration which their nature would appear to merit. These peculiarities arise partly from the mechanical action of the waters of the lake and of the rivers and streams which flow into it, and partly from certain qualities possessed by the water of the lake itself. The attention of the writer has been directed from time to time to some of these, and one of them would seem to be so much a curiosity of vegetation as to deserve particular notice. What is alluded to is the peculiar appearance presented by certain pine trees (*Pinus sylvestris*) which are found upon the east shore of the lake above mentioned. The trees present the appearance of stems supported on large aërial roots, and thus raised upon props in a mangrove-like manner. The soil is composed of about one-third common earth and two-thirds of a fine loose sand, which forms the beach of the lake, and is easily moved about by the action of running water. It is probable that, at an early stage in the



growth of these trees, they occupied a position close to the waters of the lake, but, from the gradual subsidence of the latter, they now occupy a somewhat elevated bank at a little distance from the beach. Were the singular appearance of many of them the only circumstance to draw attention, it were perhaps superfluous to make them the subject of particular remark; but there is something, in the gradual adaptation to circumstances which is traceable upon a close inspection of the specimens of vegetable life, not a little curious and interesting, as proving what the observer might be disposed to term a form of vegetable instinct, according to which the growth and stability of the tree are provided for in relation to those influences which are found in different ways to act against both. This the writer has been much struck with, after a careful examination of the present appearance of some of these trees. In the case of one specimen he has found that the height of the aërial roots from the present soil to the commencement of the trunk is upwards of five feet, and, whilst presenting thus a mangrove-like appearance not a little curious, as shown in the figure, he has found that the development of the supporting and nourishing roots seems, from period to period, to have been in close relation to the *altering exigencies* of the case. It is thus seen that, where the mechanical support came from the gradual washing away of the soil to be most needed, there the development of the means of support, and that upon plain mechanical principles, took place most prominently; and also as the tree proper came to be farther and farther elevated and removed from the soil, so the lower parts of the roots expanded and thickened, and, as it were, grasped the nourishing soil with still increasing vigour and tenacity.

2. *Remarks on* Bryum Duvalii (*Voit*), *and on the Localities in which it has been found.* By Mr John Sadler.

Bryum Duvalii is a rare and a very interesting moss. It is only about two years ago that it was recorded, for the first time, as belonging to the British flora. Wilson, in a note at page 229 of his "Bryologia Britannica," refers to this species in the following terms: "*Bryum Duvalii* may be expected to occur on the Scottish mountains, but as yet we have seen no indisputable specimens." This was in 1855. In the spring of 1858, the late Dr Nichol discovered the plant on the Moffat Hills in considerable abundance, and recorded it in the sixth volume of the Society's "Transactions" as new to Britain. In September last, the plant was again gathered in Scotland by Mr William Bell, whilst accompanying Professor Balfour on a botanical excursion to the Breadalbane mountains. He met with it growing in large patches amongst the boggy springs above Loch-na-cat on Ben Lawers.

A few weeks ago, while turning over a parcel of dried specimens of flowering plants and ferns, collected by the late Colonel Madden, near Waterford, in Ireland, and which had been transmitted to the University Herbarium about a year since by his widow, Mrs Terrot, I came upon four small tufts of a moss which, on examination, proved to be *Bryum Duvalii.* According to the date on the cover, the plant seems to have been collected in 1852, six years previous to Dr Nichol's discovery. Hence Colonel Madden was the person who first found the plant in Britain, while Dr Nichol has the merit of describing it as a Scottish species. I have no doubt that it is a moss of frequent occurrence throughout Scotland; but owing to its great resemblance to a barren state of some of our commoner species, it is very apt to be overlooked. It is remarkable that such acute observers as Greville, Hooker, Wilson, Nichol, and others, who have carefully explored Ben Lawers, should have failed to detect the plant in that locality.

The stems of the plant vary from one and a half to three inches in length, generally forming large, loosely aggregated tufts of a more or less pellucid aspect, and of a reddish-purple colour. The leaves are broadly ovate, acute, largely reticulate, and at their base strongly decurrent; their margins are entire, plane, and not thickened. It grows in wet spots, generally associated with *Bartramia fontana*, *Bryum pseudo-quetrum*, and other species. All the British specimens yet found have been barren. The capsule is somewhat pyriform and pendulous, as shown in Bruch and Schimper's figure. Mr Bell, the discoverer of the Ben Lawers specimen, has devoted much time to the examination of Scotch mosses, and, in a note to me, with reference to *Bryum Duvalii*, he remarks, "How *Bryum Duvalii* has so long escaped the notice of our British muscologists I am at a loss to understand, unless it has been mistaken for a form of *Bryum ventricosum* (*B. pseudo-quetrum*), which in some respects it resembles." Dr Asa Gray, in his "Manual of Botany," says that it resembles *Bryum turbinatum.* So far as we are aware, *Bryum Duvalii* is never found associated with *Bryum turbinatum* in Britain, but we have seen specimens of it and *B. ventricosum* growing side by side. The habitats and habits of both are alike; both have strongly decurrent leaves; the leaves of the one are broadly ovate, those of the other are oblong ovate; the capsule of the one is ventricosely pyriform, that of the other is oblong-obovate. We have little hesitation in saying that we have seen specimens of *Bryum ventricosum* that differ no more from specimens of *Bryum Duvalii* than the specimens of the latter species, collected at Waterford, Moffat, and Ben Lawers, differ amongst themselves. The Waterford specimens are more robust than either the Moffat or Ben Lawers ones; the leaves are nearly erect, ovate, and slightly acuminate. The Moffat specimens have the leaves patent, broadly ovate, with the areolation very loose. The Ben Lawers ones have the leaves recurvo-patent, broadly ovate, and the areolation close. Had the plant in question been a flowering, instead of a flowerless one, it might have been pronounced a hybrid between *Bryum turbinatum* and *Bryum ventricosum*, the latter being the female plant.

3. *Remarks on the Flora of Caithness.* By Robert Brown, Esq. of Campster.

13th December 1860.—Dr Lowe, President, in the Chair.

The following Communications were read :—

1. *The Theory of Terminal Fructification in the Simple Plant, of Ovules and Pollen, and of Spores.* By Dr Macvicar, Moffat.

(This Paper appeared in the last number of the Journal.)

2. *Tea Culture in Southern India.* By Dr Cleghorn, Madras.

3. *On the Introduction of Cinchona Trees* (Peruvian Bark) *into Southern India.* By Dr Cleghorn, Madras.

4. *Causes of the Movements in Diatoms, with illustrative models.* By S. J. Meintjes, jun., Esq.

10th January 1861.—Professor Balfour, Vice-President, in the Chair.

The following Communications were read:—

1. *On the Synonymy of* Ectocarpus brachiatus. By Dr Auguste le Jolis, Cherbourg. Communicated by Professor Balfour.

A plant, first discovered by Mr Dawson Turner in the year 1801, and afterwards noticed in 1808 by Sir William Hooker, in ditches of brackish water, near Norfolk, was described and figured in "English Botany," tab. 2571, under the name of *Conferva brachiata.* It has not been found since in that station, and it was only known by the figure in "English Botany," until Mrs Griffiths collected in Torbay an *Ectocarpus*, growing on *Rhodymenia palmata*, the characters of which seemed to correspond with those of *Conferva brachiata.* For that reason, and notwithstanding the different habitat of the latter plant, Professor Harvey figured and described, in his splendid work, "Phycologia Britannica," the marine Torbay specimens under the name of *Ectocarpus brachiatus.* He entertained, however, some doubts about the identity of the two plants, and he remarked that: "Agardh described a new *E. brachiatus*, a native of the Baltic, and conferred the name *E. cruciatus* on the 'English Botany' species. The name 'brachiatus' no doubt belongs to the Norfolk plant, and if the Torbay individuals, now figured and described, and of which excellent specimens have been published in Mrs Wyatt's 'Algæ Danmonienses,' are essentially different, a new name should be conferred on them; and Agardh's *E. brachiatus*, if it be not the same with *E. sphærophorus*, Carm., might be called *E. Agardhianus.*"

J. Agardh (Spec. Alg. i., p. 20) considered the two plants identical, and united them under the name of *E. brachiatus*, quoting as a synonym, *E. cruciatus*, C. Ag. Crouan brothers published, in their "Algues Marines du Finistère," two different species, one of which (No. 17), being the very same species as described in the "Phycologia Britannica," growing on *Rhodymenia palmata*, was named by them *E. cruciatus*, Ag.; and the other (No. 18), being a plant of brackish water, was labelled *E. brachiatus;* but this latter does not appear to be the *Conferva brachiata* of "English Botany."

My most excellent friend, the celebrated algologist, Gust. Thuret, happened to meet, on the 6th April 1857 with the true *Conferva brachiata*, E. B., growing on small stones in a ditch of brackish water on the sea-coast at Querqueville, near Cherbourg (a locality now unfortunately destroyed on account of the establishment of a polygon for the navy artillery); and this is the first instance of that plant being found in Europe since its discovery near Norfolk. After a comparison of these fresh specimens with the common parasite on *Rhodymenia palmata*, the marine species proved to be different from the brackish one. Consequently it is necessary, according to the provisions of Professor Harvey, to give distinct names to both plants, and the epithet "*brachiatus*" must evidently be retained for the Norfolk and Cherbourg brackish species, or the "English Botany" plant. It is not, however, possible to admit for the marine species the name "cruciatus" as labelled by Crouan, since C. Agardh certainly affixed that name to the "English Botany" plant, and his Baltic *E. brachiatus* is, according to J. Agardh, a mere synonym of *E. littoralis*.

As a new name is to be chosen to be conferred on the very distinct and well-marked marine *Ectocarpus* which is found along the southern coasts of England and Ireland and the northern coasts of France, growing exclusively on the fronds of *Rhodymenia palmata*, I beg to propose the name of *E. Griffithsianus*, in honour of the celebrated lady who first discovered it; and I make this proposition with more confidence, seeing that it has obtained the sanction of Professor Harvey, who wrote to me on the 1st of April 1859, in the following terms:—"I am quite contented with your name '*Griffithsianus*' for the Ectocarpus; too much honour cannot be paid to the memory of that admirable lady; and your reasons for setting aside the other names are valid."

The synonymy of the two species may be presented as follows:—

ECTOCARPUS BRACHIATUS (Engl. Bot.) = *Conferva brachiata*, Eng. Bot. t. 2571!—*E. cruciatus*, C. Ag. spec. p. 44 (*sec.* Harvey)—*non E. brachiatus*, C. Ag., *nec* Harv. Man. 1st ed., *nec* J. Ag.

In ditches of brackish water: Norfolk (Dawson Turner, 1801; Sir William Hooker, 1808); Querqueville, near Cherbourg (Gust. Thuret, 1857).

ECTOCARPUS GRIFFITHSIANUS, Nob. *in herb. E. brachiatus*, Griff.; Harv. Phyc. Brit. pl. iv., Man. ed. 2d.; J. Ag. spec. alg. I. p. 20.—*E. cruciatus*, Crouan, Alg. Mar. Finist. No. 17,—*non* C. Ag. spec., p. 44.

In the sea, growing on *Rhodymenia palmata;* England, East and South of Ireland; Northern coasts of France, Cherbourg, Brest, &c.

2. *Organogenic Researches on the Female Flower of the Coniferæ.* By Dr H. BAILLON. Memoir presented to the Académie des Sciences at its Meeting, 30th April 1860. Translated by ALEXANDER DICKSON, M.D., Edinburgh.

(This Paper appears in the present number of the Journal.)

3. *Remarks on the Theory of the Metamorphosis of Plants.* By Mr MAXWELL T. MASTERS, Lecturer on Botany, London. Communicated by Professor BALFOUR.

4. *Notice of Algæ from the Faroe Islands.* By ROBERT BROWN of Campster.

5. *On the Timbers suited for Railway Sleepers in South India.* By Dr CLEGHORN, Madras.

The author alluded to the enormous requirements of the Indian railways in the matter of timber for different purposes, especially sleepers. This is a subject of great importance, but almost the only records now existing are a suggestive paper by Dr Falconer in the Journal of the Agricultural and Horticultural Society of India, and the Jury Reports of the Madras Exhibitions of 1855–57. A portion of the immense supply required will be procured from Ceylon, Burmah, Andaman Islands, and Australia; there will also be a regular and heavy drain on the forests of South India. About thirty kinds of indigenous woods have been used experimentally for sleepers, but the trials have not been attended, in some instances, with satisfactory results; not always because the woods were inferior, but from the timber not having reached a sufficient age, and from its being employed when imperfectly seasoned. Experience has been gained, and better prospects are dawning upon the companies in this great essential of railway operations. The chief engineer, Madras Railway, lately issued a useful circular to the officers of districts regarding the branding of sleepers, of which the following is an extract:—"At the time of passing the sleepers they must be branded with a letter, showing the kind of wood according to the table of specification. Zinc labels are being prepared, which, being nailed to the sleeper, will remain legible even after its decay. These will not supersede the branding, but be used in addition to it." The results of observations by the engineers of the different railways hereafter published will be valuable; at present the summary of what is known is as follows:—Teak (*Tectona grandis*), is probably the best of all woods for sleepers, but is yearly becoming more scarce and costly. Sal (*Vatica robusta*), the next best wood, is not indigenous in any quantity south of Orissa, but is largely used in Bengal. Erul, of Malabar (*Inga xylocarpa*), is a superior wood, and suitable for railway purposes. Jarra or Yarra (*Eucalyptus rostrata*), specially noticed in a despatch from Lord Stanley, has been lately imported in considerable quantity from Swan River. A timber trade with Australia would benefit the colony, and supply the Indian market with a substitute for teak. From the monthly returns of the engineers, showing the number of sleepers delivered on the different districts of the Madras railway, and the trees furnishing them, the following results are obtained:—The largest number of sleepers are procured from the family *Combretaceæ*, and the genera *Terminalia* and *Conocarpus*, remarkable for the height and size of their trunks, and the toughness of their timber. The durability *under ground* has not been altogether satisfactory. Cadukai (*Terminalia Chebula*) appears to be liable both to the attack of fungi and of the carpenter bee. The *Leguminosæ* are next in importance, the trees supplying the wood being *Pterocarpus* (Véngé), *Inga* (Erúl), *Hardwickia* (Acha), and various species of *Acacia*. Well-grown timber of these kinds is deservedly prized by the engineers. As the native name frequently applies to a genus rather than to a species, it is doubtful whether those marked Sal and Pedowk, are really the true species, although obtained from trees belonging to the genera *Vatica* and *Pterocarpus*. The railway engineers generally have confidence in Sal, Illúpe (*Bassia*), Kara-Marda, Véngé, Chella-Wunjé. Teak being too expensive for general use as sleepers, it has recently been proposed to substitute sleepers of cast-iron for those of wood, and the plan has already been carried out on a large scale; it is thought that iron will in the end be found the most economical material for sleepers. The Indian woods, which as yet appear to be best adapted for trenails and wedges, are Kara-Marda (*Pentaptera coriacea*), and Sal (*Vatica robusta*); the former for trenails, the latter for wedges. They resist the attacks of white ants, which destroy the beech and elm imported from England.

Professor Balfour stated to the meeting, that a Botanical Society had been recently organised at Kingston, Canada West, having for its object the thorough investigation of the Canadian Flora, as well as the prosecution of the science of botany in general. The Society seems to have a similar constitution to that of the Edinburgh Botanical Society. At the first meeting, held at Kingston on Friday, 7th December last, there was a large attendance of gentlemen from all parts of the country; and, after addresses had been delivered by the Rev. Principal Leitch, Professor George Lawson, and Professor Litchfield, upwards of 100 were enrolled as members of the Society.

T. C. Archer, Esq., exhibited a hunting-net, which had been sent to the Industrial Museum by Dr Livingston from Africa, manufactured by the natives from the bark of the Baobab tree (*Adansonia digitata.*)

Thursday, 14*th February.*—Dr W. H. Lowe, President, in the Chair.

The following Communications were read:—

1. *Observations on Temperature in connection with Vegetation, having special reference to the Frost of December* 1860. By J. H. Balfour, A.M., M.D., F.R.S., Professor of Botany. *Including a Report on the Effects of the Frost on the Plants in the Royal Botanic Garden of Edinburgh.* By Mr James M'Nab, Superintendent of the Garden.

(This paper appears in the present number of the Journal.)

2. *On the Nature and Cause of the Movement of the Leaves of* Dionæa Muscipula. By S. J. Meintjes, jun., Esq.

3. *Letter from* Mr William Wilson *to* Mr John Sadler, *relative to New and Rare British Mosses.*

Paddington, near Warrington,
January 25, 1861.

Dear Sir,—The late Dr William Nichol sent me, some years ago, a small specimen of a moss which he considered to be *Atrichum tenellum*, gathered near Lochgoilhead in 1856. This I cannot find in my Herbarium, after repeated search, nor elsewhere among my papers, and I have no memorandum of having examined it.

I am particularly desirous to receive from you a duplicate of this moss (if only as a loan, for inspection and return), that I may ascertain what it really is. I hope it is *A. tenellum*, but have much doubt on the subject. It may prove to be the same as a new (British species) allied to *A. tenellum*, and which I believe we must now call *A. crispum* (Wils. MSS.), such being the name under which the moss is published (as American) in "Sullivant's Mosses and Hepaticæ of the United States;" 1856, p. 41. I had at first named it *A. laxifolium* (Wils. MSS.), for Mr James, the finder, in 1852; and Dr Schimper has it from me so named, and will very likely retain that name in his second edition of his Synopsis, if not warned in time. We have only the ♂ plant in Britain, but the fruit is found in New Jersey.

You are of course aware that *Orthotrichum anomalum* of British authors is not that of Bryol. Europæa (which is now established as a native of Britain). Our moss is *O. saxatile* of Boidel (Bryol. Univ.), and has been hitherto, on the continent, confounded with the true *O. anomalum*.

I have at last ascertained that *Campylopus longipilus* of Bryol. Europ. (also a native of Britain), is totally different from the Turnerian moss, originally named *C. longipilus* by Bridel. Schimper's moss is *C. polytrichoides* of De Notaris, and is closely allied to *C. introflexus*. It grows in Cornwall and in Jersey.*

Hypnum aduncum, var. *β*. of Bryol. Europ., is a distinct species. It is my *H. pellucidum*, MSS., found in Cheshire last year. but only the male plant, along with *H. giganteum*, Bryol. Europ.

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* In a subsequent letter, Mr Wilson states:—" *Campylopus polytrichoides* (De Not.), is *C. longipilus* of Bryol. Europ.; but not *Dicranum flexuosum*, var. *piliferum* of Turner and Smith. I have it now from Cornwall and Ireland, as well as from Jersey, where it was first found by Mrs Mackenzie."

I gathered an *Anœctangium* (but not in fruit) near Inverary in 1836, which is new and quite distinct from *A. compactum*, which was growing with it. It is my *A. pellucidum*, MSS. If you see anything of the kind in Dr Nichol's collection, with pale-yellowish foliage, scarcely at all crisped when dry, I should be glad to examine it.

I have a new *Zygodon* from Yorkshire (*Z. gracilis*, MSS., but Dr Schimper thinks it a *Didymodon*), without fruit; leaves serrate at the apex. This may be lurking in the Herbarium of your University.

A new *Orthotrichum* (*O. orneum*, Wils., MSS.) was found last summer by Mr Shaw, near Dailly.

Campylopus brevipilus seems to be generally diffused. It grows in Cornwall plentifully, also in Yorkshire, and we have it even in Cheshire. *Cinclidium stygium* has been recently found in Suffolk.

I hope the severe weather will not have destroyed the fruit of *Grimmia conferta*, &c. I have sad accounts of injury done in Cornwall to the mosses there.—Most truly yours,

W. Wilson.

Professor Balfour exhibited a gall on *Pinus sylvestris* from Finzean, Aberdeenshire, which had been given to him by William Brown, Esq., F.R.S.E., and which was caused by the attack of *Retinia resinella*.

Mr M'Nab gave the following report on the flowering of plants in the Botanic Garden:— *Eranthis hyemalis*, Jan. 26, 1861; *Crocus susianus*, Jan. 28; *Rhododendron atrovirens*, Jan. 28; *Galanthus nivalis*, Jan. 31; *Anemone hepatica*, Jan. 31; *Leucojum vernum*, Feb. 1; *Dondia Epipactis*, Feb. 1: *Sisyrinchium grandiflorum*, Feb. 5; *Omphalodes verna*, Feb. 6; *Pothos fœtidus*, Feb. 9; *Tussilago alba*, Feb. 10; *Corylus Avellana*, Feb. 10; *Crocus vernus* (yellow) Feb. 12.

Mr M'Nab noticed the flowering of *Yucca gloriosa*, var. *glaucescens*, in the garden of Captain Mitchell, Inverleith Row, Edinburgh, in September 1860. The plant was 12 feet 9 inches in height, the flower spike 5 feet 6 inches in height, and 3 feet in circumference. The leaves averaged 2 feet 3 inches in length, and 2½ inches in breadth. The specimen had been planted twenty-five years ago.

SCIENTIFIC INTELLIGENCE.

Tea Cultivation.—On the 31st of last October, the Secretary of State informed the Madras Government that he had "perused with much interest the reports by Dr Cleghorn, the Conservator of Forests, on the growth of the tea plant on the Neilgherries and other parts of the Madras Presidency." Sir C. Wood then goes on to say:—"It is satisfactorily established that tea plants will thrive in several different localities; but no attempt appears yet to have been made to convert their produce into a marketable article of commerce. I agree with you that, as a general rule, it is undesirable for Government to step out of its way to aid the efforts of private adventurers. Considering, however, the great success which has attended this branch of culture in Assam and in the Himalayas, and which, it can scarcely be expected, would have been attained, at any rate to the same extent, or in the same time, if the initiatory proceedings had not been taken by the Government, I shall not object, if it should appear that there is little chance of the matter being taken up by private enterprise, to your acting on the recommendation of Dr Cleghorn, and obtaining the services, for a limited period, of a few skilled tea manufacturers from the north-western provinces, which, it appears from the information supplied by Dr Jameson, might be procured at a very moderate cost. I presume that, under the rules now in force, the difficulties which were apparently experienced by Captain Mann in obtaining land in the Neilgherries suitable for his tea plantation will no longer exist." In an order on the above despatch, dated the 5th January, it is remarked by the Madras Government that they "have recently made a grant of land on liberal terms to Mr Rae for tea cultivation, and the advantage of their now affording assistance in this important experiment may be lessened by the fact of its having been thus undertaken by a private individual. The Board will, however, consult the collector of Coimbatore and some of the residents of the hills on the subject."—*Madras Athenæum.*

Slate in India—Mr Oldham, superintendent of the Geological Survey of India, has written a long memorandum on the use of slates in India generally, and on the slabs of the Kurnool district particularly. After explaining at length the nature of true slate, namely, that it is "capable of almost infinite division, thin plates or slabs splitting with tolerably even surfaces of considerable size," he goes on to show that the Kurnool slabs and the same material found in other parts of India are incapable of this infinite division, &c. And he is of opinion that "the Kurnool slabs referred to by Lieutenant Beckley and the Madras Government are entirely unfitted for sloping roofs; that they cannot be procured in slabs dividing naturally of such size and thickness as would adapt them for such roofs; that sawing them would, even if practicable, be too expensive; that the slabs thus procured would be either too thin to give the requisite strength, or, if of sufficient strength, would be too heavy and thick for economical or effective use." But for flat roofs or floors he thinks they may be used with advantage. Mr Oldham adds:—"I would further urge that such stone slab floors, where the proper material can be procured with a moderate amount of carriage, and at a fairly reasonable rate, will prove much more durable, economical, more cleanly, and in every respect better floors than either wood or 'pucka' for barracks, hospitals, courthouses, or any place where there is constant intercourse, and also for the verandahs of such buildings. I have just alluded to the cleanliness of such floors; and I consider this to be by no means a trifling advantage. They can be mopped out with clean water or washed with soap and water in the same way as ordinary wooden floors, and can thus be kept sweet, clean, and free from vermin with the smallest amount of labour. There are several localities in Bengal and the north-western provinces where such slabs could be obtained as would be suited for flooring. The hills to the south of Monghyr, the Sikkim Hill (poor), the Soane Valley, the Kumaon Hills, and the Gwalior Hills. But in few cases will such materials admit of any great length of carriage; and they can therefore only be used economically when procured within a reasonable distance of the works where they are required."—*Indian Mail, 13th February* 1861.

New Canadian Dye.—At a meeting of the Botanical Society of Canada on 15th February last, Professor Lawson exhibited specimens of a new dye of great richness, prepared in the laboratory of Queen's College, from an insect, a species of Coccus, found for the first time last summer on a tree of the common black spruce (*Abies nigra*, Poir), in the neighbourhood of Kingston. This new dye closely resembles true cochineal, a most expensive colouring matter capable of being produced in warm countries only, and which is used to give a fine and permanent dye in red, crimson, and scarlet, to wool and silk. Unlike cochineal, the new dye, discovered at Kingston, is a native Canadian product, and capable of being produced in temperate countries. Having been but recently observed, a sufficient quantity has not yet been obtained for a complete series of experiments as to its nature and uses; but the habits of the insect, as well as the properties of the dye, seem to indicate that it may become of practical importance. In colour it closely resembles ordinary cochineal, having rather more the scarlet hue of the flowers of *Adonis autumnalis*, and no doubt other shades will be obtained. The true Mexican cochineal is now being cultivated in Teneriffe and other vine growing countries of Europe and Africa, with such success as to displace the culture of the grape vine; yet the Directors of the East India Company offered in vain £2000 for its introduction into India.

Magnesium as a Source of Light.—Professor A. Schmitt calls attention to the practicability of employing metallic magnesium for purposes of illumination, as had already been suggested by Bunsen. From the researches of the last named chemist, it is known that when magnesium is ignited it readily takes fire and burns with an exceedingly brilliant flame. The intensity of the light thus produced, as determined by Bunsen and Roscoe in one of their photo-chemical researches (*Pogg. Annalen*, cviii., 261, *et seq.*) is only some 525 times less than that of the sun. Compared with an ordinary candle, it appeared that a wire of magnesium 0·297 millimetre [1 mm.=0·0394 inch] in diameter, produced as much light in burning as 74 stearine candles, five to the pound. In order to support this light during one minute, a piece of wire 0·983 metres long, weighing 0·1204 grm. [1 grm.=15·4325 grains], was required.

Only 72·2 grms. of magnesium, therefore, would be needed, in order to maintain during ten hours an amount of light equal to that of 74 stearine candles, consuming about 10·000 grms. of stearine.

According to Bunsen, magnesium wire is readily obtained, by forcibly pressing the metal through a hot steel die by means of a steel piston. Bunsen's arrangement for burning the wire was made by connecting spools of it with rollers moved by clock-work, so that the work should be unrolled like the ribbon of paper in Morse's telegraph, the end of the wire thus gradually pushed forward, passed into the flame of an ordinary alcohol lamp, where it took fire.

It is evident that a magnesium lamp of this sort must be much simpler and more compendious than any of the existing arrangements of the electrical, or of Drummond's light. For light-houses, &c., where an intensely brilliant illumination is required it can hardly fail to rival either of these. Where an extraordinary amount of light is needed, it could readily be produced by burning large wires, or several thin ones at the same time. Another important consideration is the fact that the spools of wire, as well as the clock-work and spirit-lamp, are easily transportable.

It is not, however, to the intensity alone of the magnesium flame that these lamps owe their utility, for the photochemical (*i. e.*, photographical) effect of the light is also very great. According to Bunsen, the photochemical power of the sun is only 36·6 times greater than that of the magnesium flame. The latter must therefore be useful in photographing by night or in any dark or subterranean locality; the evenness and remarkable tranquillity of the flame, especially commending it for this purpose.

The present high price of magnesium, it is true, must prevent any extended use of it for technical purposes. For example, Lenoir of Vienna charges 3 Florins [1 Fl.=51 cts.] for a grm. of it; hence the cost per minute of the light just described would be 36 *Neukseutzer* [1 ktr.= about ½ of a ct.]; and the cost during ten hours, would amount to 216 Florins, while the ten kilogrammes of stearine could be procured for less than 14 Florins. But even at this price, it could still be used by photographers, since it would only be required for exceedingly short intervals of time, and all unnecessary consumption of the wire might be prevented by stopping the clock-work.—From *Stamm's Illustr. Zeitschrift*, 1859, p. 332; in *Polytechnisches Notizblatt*, 1860, xv. 56.

ABSTRACT OF THE METEOROLOGICAL REGISTER FOR 1860,

Kept at Arbroath, by **ALEXANDER BROWN**, Honorary Member of the Literary and Philosophical Society, St Andrews; Observing Member of the Scottish Meteorological Society, &c.

Latitude 56° 34′ N. Longitude 2° 35′ W. Distance from the Sea, ⅛ths of a Mile.
Height of the Barometer above the Sea, 75 feet; height of the Thermometer from the ground, 11 feet, and of the Rain-Gauge, 2 feet.
The number of "Rainy Days" includes those on which snow or hail fell.

1860.	BAROMETER.		THERMOMETER.							Days Ther. below 32°.	Rain in Inches.	HYGROMETER.		Fair Days.	Rainy Days.	WINDS, AT 8½ A.M.									
	Corrected to 32°, and reduced to sea-level.		8½ A.M.	7¾ P.M.	Mean.	Mean Max.	Mean Min.	Mean.	Spring Water.			Mean Dew Point.*	Degree of Humidity (complete Sat. 1·000).			N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.	Total.
	8½h. A.M.	8½h. P.M.																							
January,	29·46	29·48	32°·9	33°·1	33°·0	41°·2	29°·3	35°·2	47°·2	23	3·415	30°·2	0·906	16	15	1	3	1	5	3	4	4	9	1	31
February,	29·84	29·86	30·4	31·9	31·1	37·5	27·1	32·3	46·5	24	1·046	25·1	·825	23	6	3	1	—	—	1	4	5	12	3	29
March,	29·64	29·61	35·9	35·9	35·9	43·4	30·8	37·1	47·0	24	1·956	31·0	·838	21	10	2	—	—	2	2	4	13	6	2	31
April,	29·91	29·93	42·0	41·0	41·5	48·9	34·3	41·6	46·5	12	0·778	35·0	·785	21	9	3	—	5	2	2	1	6	9	2	30
May,	29·78	29·74	51·1	50·0	50·6	57·3	44·4	50·8	46·7	—	1·184	43·0	·760	19	12	1	2	9	4	3	—	7	1	4	31
June,	29·60	29·61	53·8	51·7	52·7	59·8	48·3	54·0	48·0	—	4·752	47·2	·787	16	14	1	2	12	5	1	3	1	2	3	30
July,	29·91	29·89	58·3	58·6	58·5	64·8	53·2	59·0	48·7	—	1·875	51·0	·783	24	7	6	1	11	—	1	—	—	6	6	31
August,	29·50	29·50	56·1	56·1	56·1	63·3	49·3	56·3	49·0	—	2·960	49·2	·798	14	17	2	1	2	3	2	4	2	10	5	31
September,	29·78	29·79	50·8	50·7	50·8	59·4	44·3	51·8	50·0	—	1·036	45·0	·814	21	9	3	1	1	1	—	—	4	14	6	30
October,	29·70	29·74	45·9	45·9	45·9	51·2	41·1	46·1	49·7	1	2·508	40·7	·833	14	17	3	—	1	2	1	3	8	8	5	31
November,	29·84	29·85	39·8	40·3	40·1	44·7	37·2	40·9	49·0	7	4·327	35·6	·858	13	17	2	—	7	6	1	2	3	5	4	30
December,	29·61	29·61	34·7	34·5	34·6	38·8	32·0	35·4	46·0	13	4·638	31·7	·900	14	17	2	1	2	7	1	2	2	10	4	31
Mean,	29·72	29·72	44·3	44·1	44·2	50·8	39·2	45·0	47·8	104	30·475	38·7	0·823	216	150	29	12	51	37	18	27	55	92	45	366
Do. 1859,	29·79	29·79	46·0	45·4	45·7	52·3	39·4	45·8	48·5	103	25·378	40·7	0·836	230	135	16	23	30	35	36	48	89	47	41	365

Barometer at 8½ A.M. was highest on 13th February, 30·68.† Wind N.E.
Do. do. was lowest on 24th January, 28·45. Wind N.W.
Do. at 8½ P.M. was highest on 13th February, 30·70. Wind N.E.
Do. do. was lowest on 21st January, 28·42. Wind S.E.
Thermometer at 8½ A.M. was highest on 3d July, 65½°. Wind N.W.
Do. do. was lowest on 25th December, 12°. Wind N.W.
Do. at 7¾ P.M. was highest on 5th July, 67°. Wind N.W.
Do. do. was lowest on 25th December, 12°. Wind N.W.
Thermometer in night, highest, 4th and 6th July, 58°; lowest, 26th December, 11°.
Thermometer in day, highest, 5th July, 76°; lowest, 25th December, 19°.
Coldest day, 25th December, when average of Thermometer was 15½° for day and night.
Hottest days, 1st, 3d, and 6th July, when average of Thermometer was 63° for day and night.
Coldest month of the year, February; hottest, July.
Wettest month of the year, December; driest, April.
Mean temperature of the year, 44·6 degrees.
Mean temperature of seventeen years, 45·629 degrees.

For twelve years the average daily temperature at 8½ A.M., at 7¾ P.M., and the mean of the daily extremes, are as follows, viz.:—

	8½ A.M.	7¾ P.M.	Mean of Extremes.
1849	45°·1	44°·4	44°·7
1850	45 ·5	44 ·9	45 ·5
1851	45 ·7	45 ·4	46 ·3
1852	46 ·0	45 ·9	46 ·2
1853	45 ·4	44 ·9	45 ·6
1854	46 ·3	46 ·4	46 ·3
1855	44 ·2	43 ·9	44 ·3
1856	44 ·9	44 ·5	45 ·0
1857	48 ·2	47 ·9	48 ·1
1858	46 ·3	46 ·3	46 ·4
1859	46 ·0	45 ·4	45 ·8
1860	44 ·3	44 ·1	45 ·0

The average temperature of the six months of chief vegetation, viz., those from April to September inclusive—for twelve years, is as follows:—

1849 ... 52°·2	1851 ... 52°·0	1853 ... 53°·4	1855 ... 52°·1	1857 ... 54°·7	1859 ... 52°·9
1850 ... 51 ·7	1852 ... 53 ·0	1854 ... 53 ·5	1856 ... 50 ·9	1858 ... 53 ·5	1860 ... 52 ·3

---

* The dew-point thus found is obtained from observations of the Wet and Dry Bulb Thermometers, and deduced by Mr Glaisher's Hygrometrical Tables. The observations of the Wet and Dry Bulb Thermometers are made daily at 8½ A.M. The times of observation of the instruments stated in the Tables are Greenwich mean times.

† The highest and lowest readings of the Barometer are reduced to sea-level, and otherwise corrected.

Stelliform Crystals of Snow.—In an article on Stelliform Crystals with special reference to the crystallization of snow, Professor Chapman of Toronto, comes to the following conclusions:—(1.) Stelliform six-rayed crystals are common to various systems, occurring not only in the trimetric and in the monometric systems, new examples of which (as regards the latter) are described; but also in the monoclinic system, in which, until now, none have been announced; and in which, moreover, by some observers, these have been thought of impossible occurrence. (2.) Although thus shown to occur in various systems, none have yet been recognised with certainty, amongst minerals or in artificial crystals of the hexagonal system. (3.) Hence, from the facts given in conclusions 1 and 2, the assumed hexagonal crystallization of snow, if not disproved, becomes at least of very doubtful acceptation.—*Canadian Journal, January* 1861.

A Lunar Tidal Wave in Lake Michigan.—Lieut.-Colonel J. D. Graham has announced the occurrence of a semi-diurnal lunar tidal wave in Lake Michigan. His statement is based on 9184 observations, made on the tide-guage at Chicago, of the elevation of the surface of this lake, between the 1st of January and the 1st July 1859. The observations were carried on uninterruptedly both day and night (except in a few instances when violent storms would have rendered them inaccurate), at intervals of half an hour, as a general rule, and sometimes at intervals of fifteen minutes of time apart. From this series of observations was deduced the half-hourly (and in two places the quarter-hourly) co-ordinates of altitude of the lake surface, compared with the time (before and after) of the moon's meridian transit.

PUBLICATIONS RECEIVED.

American Journal of Science and Arts, September 1860.—*From the Editors.*

Dublin Quarterly Journal of Science, No. 1, for January 1861.—*From the Editor.*

The Canadian Naturalist and Geologist, for December 1860.—*From the Editors.*

The Chemist and Druggist for February 15, 1861.

Lunar Tidal Wave in the North American Lakes. By Brevet Lieut.-Colonel J. D. Graham.—*From the Author.*

Journal of the Academy of Natural Sciences of Philadelphia. New Series, Vol. IV., Part 4, December 1860.—*From the Academy.*

Proceedings of the Academy of Sciences of Philadelphia for 1860, page 285 to page 579.—*From the Academy.*

Notice of the Origin, Progress, and Present Condition of the Academy of Natural Sciences of Philadelphia. By W. S. W. Ruschenberger, M.D. —*From the Author.*

Geological Survey of Canada.—Report of Progress for the year 1858. —*From the Director of the Survey.*

The Mathematical Works of Isaac Barrow, D.D. Edited by the Rev. Dr Whewell.—*From the Editor.*

The Genetic Cycle in Organic Nature. By George Ogilvie, M.D., Professor of the Institutes of Medicine, Aberdeen.—*From the Author.*

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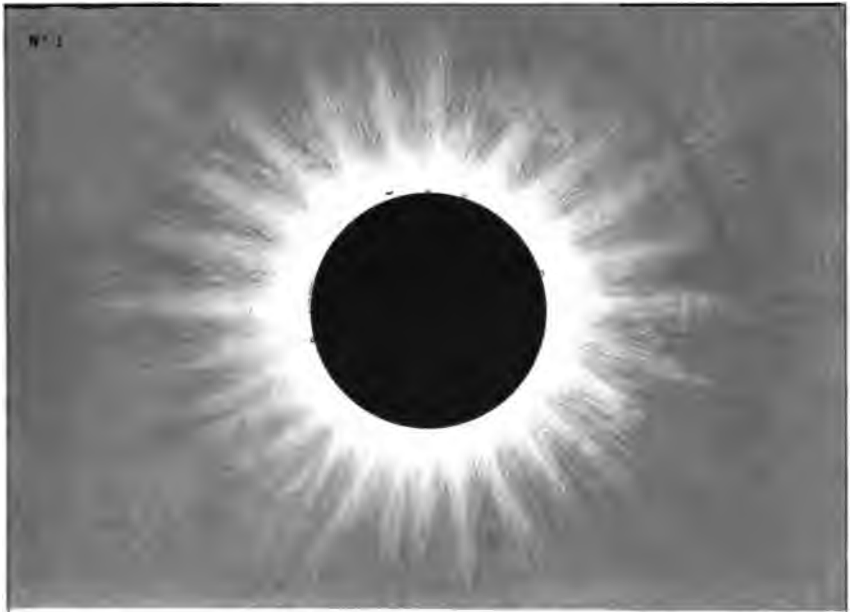

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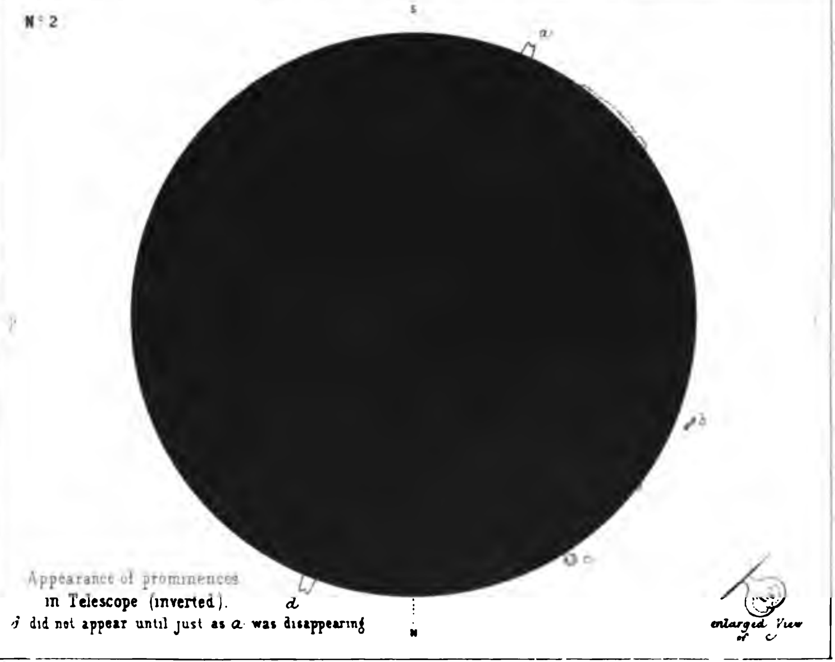

Appearance of prominences in Telescope (inverted).

d did not appear until just as a was disappearing

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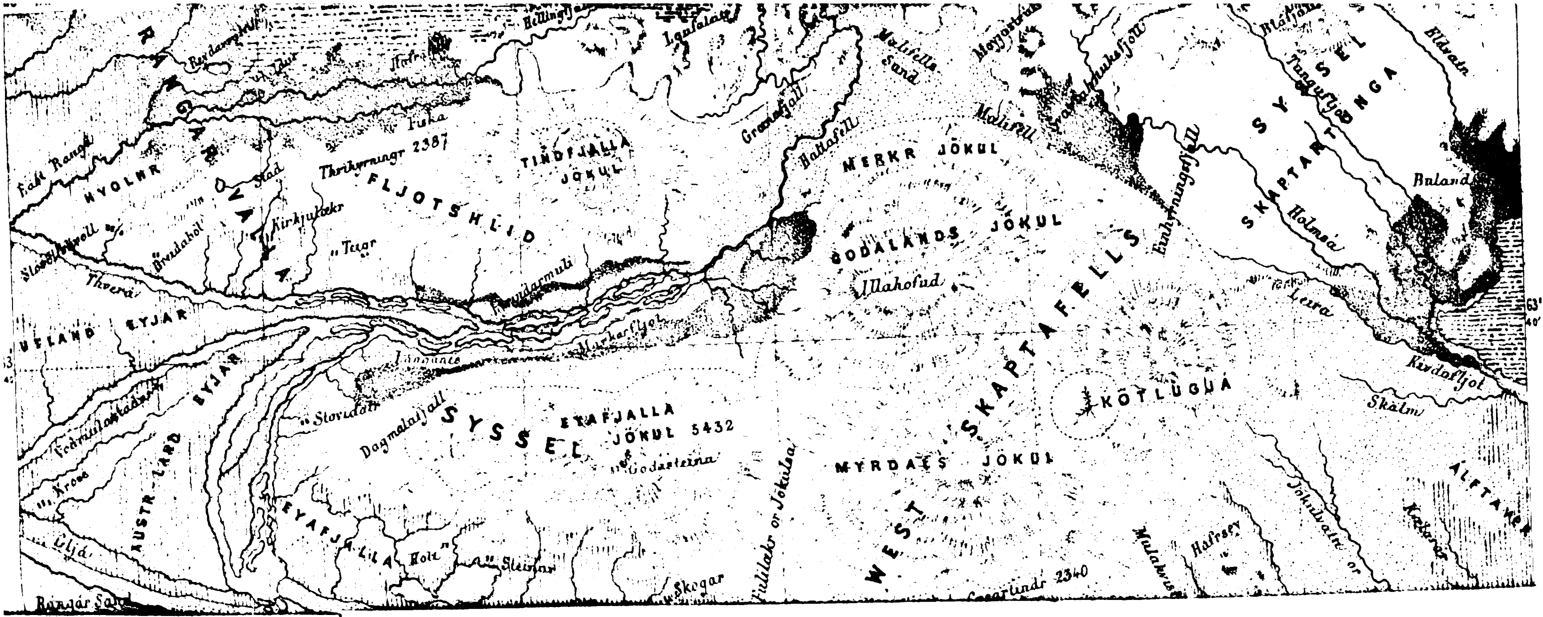
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Skálm
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Leira
Holmsá
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Tungufljot
Blafjall
Eldvatn
63°
40'

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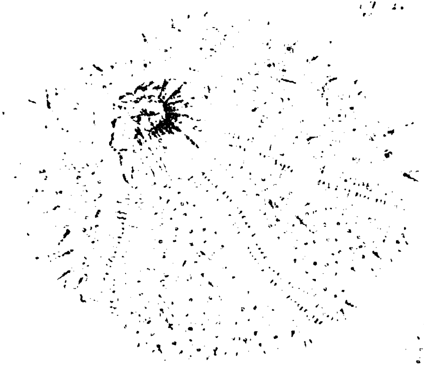

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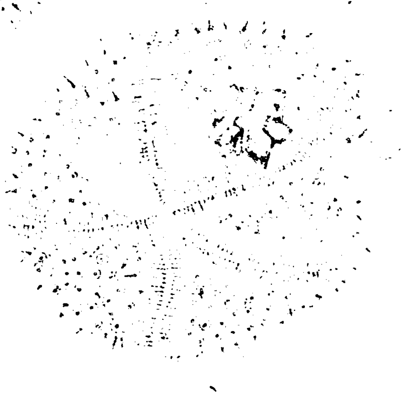

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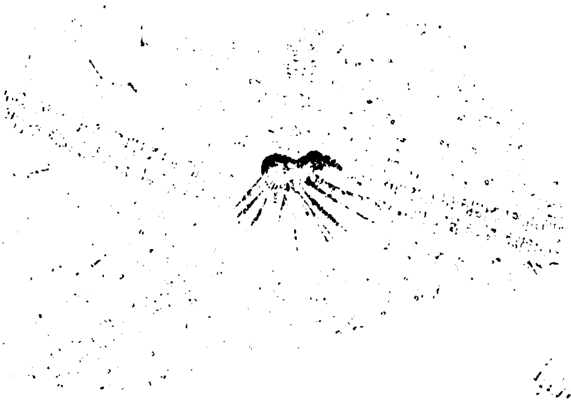

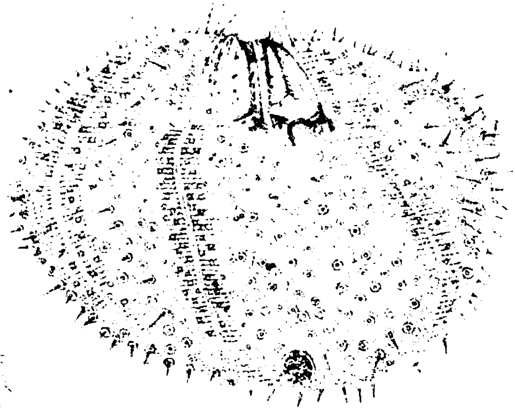
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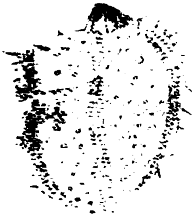

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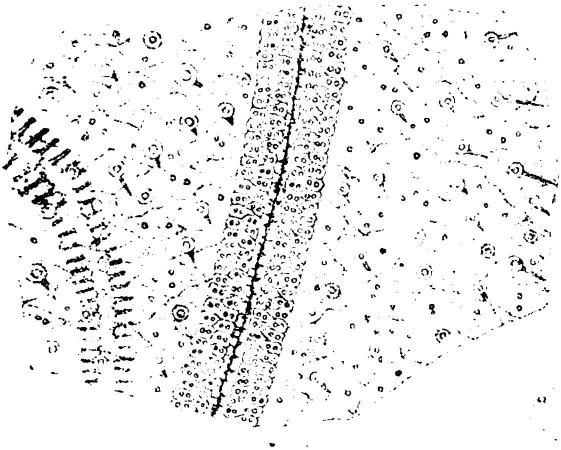
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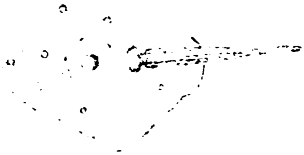
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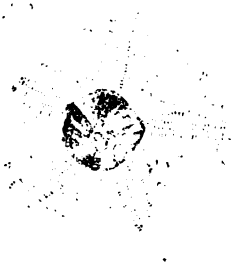

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Fig. 7

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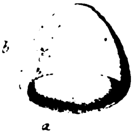

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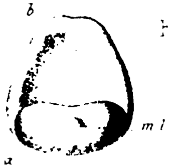

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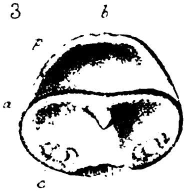

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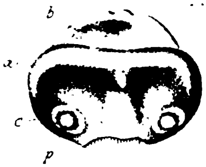

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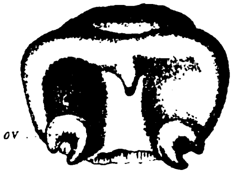

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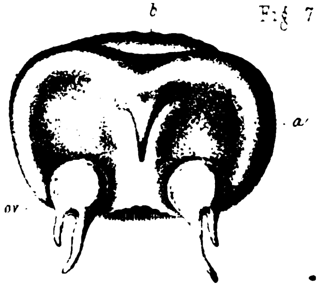

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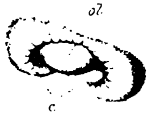

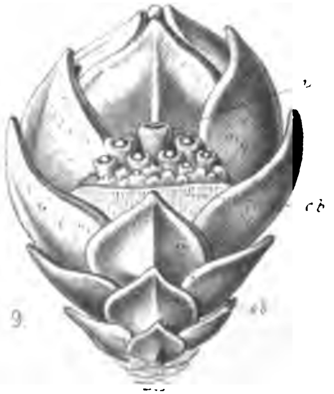

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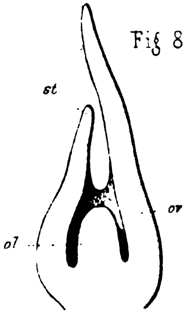

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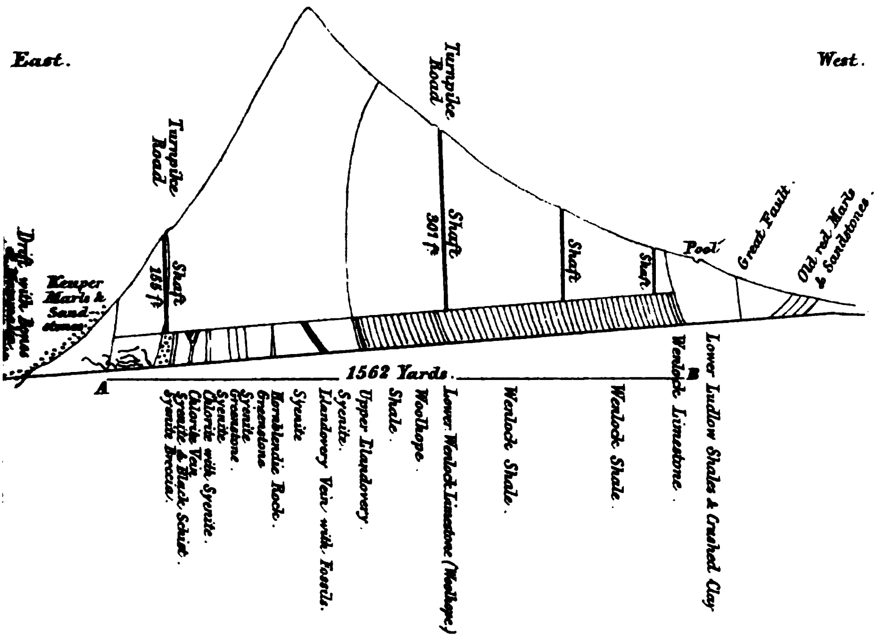

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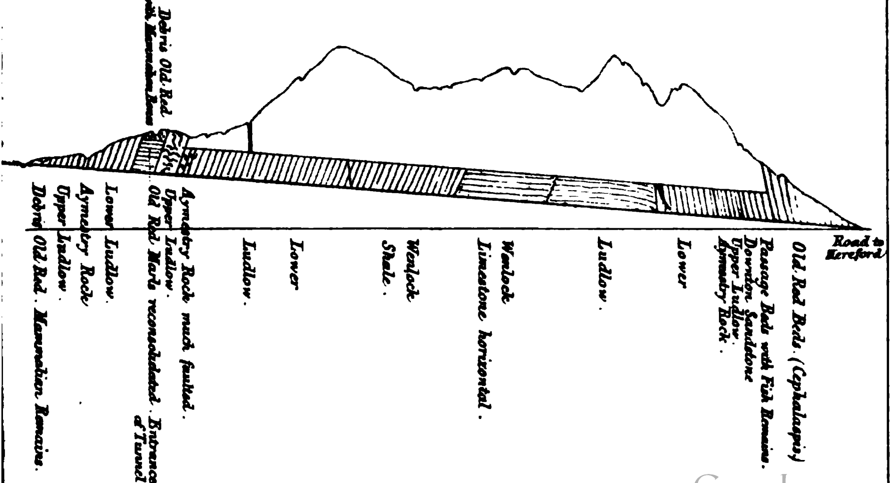
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Debris Old Red. Mammalian Remains.
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Aymestry Rock
Lower Ludlow.
Old Red Marls reconsolidated. Entrance of Tunnel
Upper Ludlow.
Aymestry Rock much faulted.
Ludlow.
Lower
Shale.
Wenlock
Limestone horizontal.
Wenlock
Ludlow.
Lower
Aymestry Rock.
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